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DARWINISM AND LAMARCKISM

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Lamarck

(1744–1829)

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F. Jenkins, Helicg.

DARWINISM AND LAMARCKISM

OLD AND NEW

FOUR LECTURES

BY

FREDERICK WOLLASTON HUTTON
F.R.S. &c.

LONDON
DUCKWORTH & CO.
3 HENRIETTA STREET, COVENT GARDEN, W.C.
1899

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APHORISMS BY GOETHE

(Translated by the Right Hon. T. H. Huxley)

NATURE! We are surrounded and embraced by her: powerless to separate ourselves from her, and powerless to penetrate beyond her.

Without asking or warning, she snatches us up into her circling dance, and whirls us on, until we are tired and drop from her arms.

She is ever shaping new forms: what is, has never yet been; what has been, comes not again. Everything is new, and yet nought but the old.

We live in her midst, and know her not. She is incessantly speaking to us, but betrays not her secret. We constantly act upon her, and yet have no power over her.

. . . . . .

Quietude is inconceivable to her, and she has laid her curse upon rest.

. . . . . .

She wraps man in darkness, and makes him for ever long for light. She creates him dependent upon the earth, dull and heavy; and yet is always shaking him until he attempts to soar above it.

. . . . . .

Everyone sees her in his own fashion. She hides under a thousand names and phrases, and is always the same. She has brought me here, and will also lead me away. I trust her. She may scold me, but she will not hate her work.

Nature, vol. i., pp. 9-10.

PREFACE

THE first two of these lectures were given to the members of the Philosophical Institute of Canterbury, New Zealand, on the 12th September, 1887, and the 21st September, 1898, respectively. Of the first a small edition was printed for private circulation, and is now republished with only a few verbal alterations, as, by the nature of the subject, it is not necessary to keep it up-to-date. On the contrary, it appears to me to be preferable that it should reflect the ideas of naturalists at the time it was written. The second lecture is printed for the first time. The third was given to the students of Canterbury College, University of New Zealand, as the opening address of the session on the 20th March, 1882, and was subsequently printed in the *New Zealand Journal of Science,* under the title of "Biology in an Arts Curriculum." I have omitted the opening and closing paragraphs; but the rest is substantially the same as when given. The fourth and concluding lecture, on "The New Lamarckism" was written to make my treatment of the subject more complete.

My excuse for adding to the already voluminous literature on Darwinism is that the subject is always advancing, and that the interest attached to it is not confined to naturalists, but enters into every-day life. It is, indeed, intimately connected with our systems of theology, for it forms one of the foundations—perhaps

the corner stone—of Natural Religion; by which I mean the religious ideas and beliefs we receive from a study of nature.

It is therefore important that a knowledge of the theory should be widely spread; and any attempt to convey that knowledge in simple language can hardly fail to do good, provided it be sufficiently clear to be understood at the first reading, and sufficiently short to discourage skipping.

This has been my aim; and, as all the first three lectures were addressed to mixed audiences, and were intended to be popular expositions of the Darwinian doctrine, they may perhaps find favour with those who have no time to study more elaborate works.

F. W. H.

CHRISTCHURCH, NEW ZEALAND,
January, 1899.

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INTRODUCTION

In 1887, when the first of these lectures was given, Darwinism was a compact body of doctrine, obscured only by the writings of certain philosophers who imagined that natural selection was a cause of variation, in spite of the expressed statement to the contrary which Darwin found it necessary to make, eight years after the publication of the "Origin of Species." "Natural selection," he said, "has no relation whatever to the primary cause of any modification of structure." [1]

Darwin certainly investigated the problems of inheritance and variation, and added largely to our knowledge of the facts which have still to be explained. But he always admitted that he had failed to penetrate the mystery behind them; and, as he published his speculations on these subjects in a separate book, he cannot be accused of trying to incorporate them with his theory of development which is contained in the "Origin of Species." In this book he has a chapter on the laws of variation, in which he says that our ignorance of them is profound. He makes no attempt to explain them; nor does he propose any hypothesis of the origin of variations. On the contrary, he says, "whatever the cause may be of each slight difference in the offspring from their parents—and a cause for each must exist—it is the steady

[1] "Animals and Plants under Domestication." 2nd ed. Vol. ii., p. 272.

accumulation, through natural selection, of such differences, when beneficial, that gives rise to all the more important modifications of structure, by which the innumerable beings on the face of the earth are enabled to struggle with each other, and the best adapted to survive." This, which was published in 1859, is strictly correct at the present day.

In 1899 things are different. The confusion just alluded to has much increased. Conceptions totally irrelevant to Darwinism have been fastened on it, and all kinds of misconceptions have grown up. Indeed, things have fared so badly since Darwin's death that I have seen it stated that his flock has scattered, and that the great theory he so successfully reared is in danger of falling to pieces.

This, however, is a mistake, founded on an erroneous conception of what Darwin's work really was. Of course things have changed. New facts have been discovered, new reasonings have been produced, and we have lost the leader who used to sift the facts and reasonings for us. Under these circumstances a few scientific agnostics have arisen, who declare they doubt everything; but I fancy that most naturalists know their own minds, and have their own beliefs.

OLD OBJECTIONS TO DARWINISM

In the early days of Darwinism the objection—long since abandoned—was often urged against natural selection, that it was impossible to suppose that the few individuals, which possessed any advantageous structure in the highest degree, would pair and transmit it to their offspring undiminished by intercrossing. No doubt Darwin at first thought that single varia-

tions could be thus perpetuated by natural selection; but so long ago as 1867 he abandoned that view, after having read an article by Mr Fleeming Jenkin, in the *North British Review*, which convinced him that free intercrossing must necessarily destroy the effect of any single variation. He says that his mistake arose from thinking too much of the cases of selection by man.[1] He then recognised that natural selection acts, not by preserving a few highly favoured individuals, but by killing off all those which do not come up to a certain standard. Its mode of action is not like that of leading a horse, but like that of driving a flock of sheep. It is not in any way analogous to the methodical selection of man, but more nearly represents what Darwin called unconscious selection.

This objection, which was supposed to be dead and buried, was resuscitated by the Marquis of Salisbury in his Presidential Address to the British Association at Oxford in 1894. He asked, "What is to secure that the two individuals of opposite sexes in the primeval forest, who have been both accidentally blessed with the same advantageous variation, shall meet and transmit by inheritance that variation to their successors? Unless this step is made good, the modification will never get a start; and yet there is nothing to insure that step but chance." This difficulty, however, arose entirely from a mistaken view of the real mode of action of natural selection; and so soon as that mistake was seen the difficulty vanished.

Lord Salisbury also brought forward the further objection that physicists had shewn that the earth had

[1] "Origin of Species," 5th ed. (1869), p. 104; and "Life and Letters," iii. pp. 107-8.

not existed long enough to allow of the process of development by the Darwinian theory, as that process was necessarily a very slow one. This also is a misconception of Darwinism. The objection is a mild form of the very venerable one which Cuvier brought against the theory of Lamarck in the beginning of the century, and which is mentioned in the first of these lectures. The answer is that if organic development has been slow during the last three or four thousand years, so also has been the geological development. As the earth has, as a matter of fact, existed long enough for the geological evolution to work out, so also must it have existed long enough for the organic evolution; for all through the earth's history the two have gone together. From the Radiolarians and sponges of the Huronian, through the Trilobites and Brachiopods of the older Palæozoic, the fishes and land-plants of the newer Palæozoic, the reptiles and Gymnosperms of the Mesozoic, the birds, mammals and angiosperms of the Cainozoic, the geologic and biologic evolutions have marched on hand in hand; and, as there has been time for one, there must have been time for the other. The rate of evolution does not affect Darwinism, which has nothing to do with the origin of varieties. If the varieties came quickly, natural selection would act quickly; and *vice versa*. No doubt, if the physicists are right, variation must have gone on quicker than it does now; but Professor Poulton has shown that the data on which physicists have calculated the short history of the earth are untrustworthy, and not entitled to so much weight as the facts brought forward by geologists.[1]

[1] British Association Report for 1896, p. 808.

Of course we do not expect Lord Salisbury to be up-to-date in biological theory; but, as his address will probably be memorable as the last attack on Darwinism from the Presidential Chair of a scientific society, and as it contains what he evidently thought was a death blow to the whole theory, I feel obliged to mention it here.

The first thing to do, in order to understand Darwinism, either old or new, is to dismiss from our minds any idea that it goes to the root of things and attempts to explain origins. The truth is that the new Darwinism is merely an expanded form of the old, in which, in addition to selection, isolation is shown to be necessary for organic evolution; and this, in my opinion, is the only real advance that has been made since Darwin's death. The new teaching explains the facts much better than the old; but there still remains much to be desired, especially in the collection of evidence to test the truth of those parts of the theory which relate to physiological isolation.

IDEA OF AN INTERNAL TENDENCY TO PROGRESSION

It will be noticed that in the first lecture it is pointed out that natural selection does not afford a complete solution of the problem of organic evolution; but the new teaching is not even mentioned: for, plain as it now seems, it was not very convincing in its early days. On the contrary, it is there stated that "many biologists are of opinion that there is an inherent tendency towards higher organisation; they think that protoplasm tends to become more and more complex, and that evolution is the inevitable outcome of a fundamental property of living matter." The

idea of an internal force, compelling development in certain directions, and especially towards higher organisation, which was common eleven years ago, is still held by some naturalists; but it has almost vanished before the accumulating testimony of palæontology, that several of the lower organisms have existed from the earliest part of the Palæozoic era up to the present day, without undergoing any important change in their hard parts—which alone have been preserved—and therefore probably without much change in their soft parts either.

Indeed, most of the classes of animals and plants have undergone but little change since their first appearance. In comparatively few cases has change been rapid; but it is these rapidly changing forms which seem so remarkable in our eyes, and give the impression that great change is more universal than it really is. Except the large Lycopods and Crustaceans of the newer Palæozoic, the reptiles and birds of the Mesozoic, and perhaps a few of the Eocene hoofed mammals, there is nothing among extinct plants and animals that would appeal to the untrained eye as anything remarkable and unlike living plants and animals. Our views on this subject are much exaggerated, owing to the numerous drawings and models that have been made of a few of the most extraordinary of the animals; and we forget that they were only a few among a host of quite ordinary beings.

DEFINITE AND INDEFINITE VARIATION

In the second lecture I have entered somewhat fully into the difference between Lamarckism and Darwinism, because I have noticed misconceptions on

the subject among several writers on evolution; and it was necessary to make the difference as clear as possible, without going into technical details. There seems to be especial confusion on the subject of definite or determinate, and indefinite or indeterminate variation; and a few more words about it may not be out of place in this Introduction; for, although not included in Darwinism, it has an indirect bearing on our views of natural selection.

Some writers seem to think that because determinate variation implies a directing agency, therefore indeterminate variation can have no directing agency, and that it has no cause at all. This confusion is largely due to the unfortunate substitution of the terms "determinate" and "indeterminate" for Darwin's "definite" and "indefinite" variation, which are far more appropriate. I have also seen it stated that, if variations are definite, there is nothing left for natural selection to do; which is quite a mistake. A definite variation may be injurious, or it may be indifferent, or it may be useful to its possessor. The first will be checked, the second will be left alone, and the third will be still further developed by natural selection. The mistake has arisen through supposing that all definite variations must be useful, which would no doubt be the case if all definite variations were due to use-inheritance; but we have no reason to suppose that this is so. This is clearly seen when we consider mental variations, many of which are definite, that is, are impressed simultaneously on a large number of individuals, but are subject to the laws of selection, as is pointed out in the third lecture.

"Definite variation" only means that a variation,

gradually increasing in intensity, is transmitted by a number of individuals from one generation to the next. While "indefinite variation" means that each variation is individual only, and may or may not die out with the individual. Individual variations are often transmitted from one generation to another; and the oftener they are transmitted, the more constant they become; until, at last, a large number of individuals constantly acquire the same character. Here we have cases of indefinite changing into definite variation, showing that the two are fundamentally the same; the difference between them lying in the strength and constancy of the directing cause.

Variations must be due either, (1) to the action of the environment, directly on the individuals, or indirectly, by causing a change of habit, and thus leading to the greater or less use of certain organs. Or, (2) to internal causes affecting the action of some law of growth which counteracts the law of heredity. Each of these causes may possibly give rise to indefinite or to definite variation, according to the strength with which it acts. We have no reason to suppose that external causes must act identically on different individuals, or that because one individual changes its habits therefore many must do the same; but no doubt the environment, if it acts at all, would generally affect a large number of individuals simultaneously, and post-natal variations would generally be definite. On the other hand, indefinite variations would generally arise during the development of the individual; and consequently they must generally be congenital in origin, although the effects may not show until long after birth.

Definite variation is due to the directive force being sufficiently strong to overcome all obstacles. The greatest obstacle is free intercrossing with individuals which do not possess the variation. This is overcome either by the isolation of a few individuals, or by a large number of individuals changing together in the same direction, and so forming a group by themselves. This last is the necessary foundation of Lamarckism; and the term "determinate variation" might be restricted to it, provided it was well understood that determinate variation was only a special form of definite variation. The first is the foundation of the new Darwinism; but that theory does not exclude definite variation, or even that particular form of it which we have just called determinate variation, if it should hereafter be shown that such variations can be transmitted from one generation to another. Indeed, it is well known that Mr Darwin allowed far more influence to use-inheritance than the new Darwinians are inclined to do.

THE TRANSMISSION OF ACQUIRED CHARACTERS

That post-natally acquired mental characters or variations can be transmitted, we know from many undoubted cases of inherited habits; but it does not necessarily follow that post-natally acquired structural characters can be transmitted. For mental variations are transmitted in two ways: first by imitation or education, and secondly by inheritance. It is probable that the second form can only occur after the variation has been transmitted by imitation through several generations, during which it passes from the indefinite to the definite stage.

This cannot take place with structural variations; and a post-natally acquired character has but a small chance of becoming a congenital character, unless it is reproduced generation after generation by the action of climate or other external agent. It might then perhaps in time become definite and congenital, provided there is some process by which structural variations affect the germ-cells in a similar manner to the action of mental variations on the brain-cells, which—although we cannot explain it—we know to be a fact by the phenomenon of memory. And, as something very similar to memory appears to be the cause of structural development, this may be the case; the evidence bearing on the subject, however, is weak and contradictory. Mr Darwin offered an explanation in his hypothesis of Pangenesis; but, for reasons given in the first lecture, that hypothesis cannot be maintained, at any rate in its integrity.

It is thought by some that "if natural selection be inadequate to explain many of the facts of evolution, there is no alternative but the view that development is partly caused by the transmission of changes brought about in the organism as a result of its own activity, directed and conditioned by the environment, and of the action of external agencies."[1] Which, I suppose, means that there is no alternative but a partial return to Lamarckism. It will, however, be seen in the second lecture that there is no necessity for thinking that acquired characters must be inherited in order to supplement the action of natural selection. The isolation of variations, no matter how they have arisen, is sufficient to explain all the deficiencies without any

[1] Parker and Haswell, "Text-book of Zoology," vol. ii., p. 627.

help from Lamarckism. Indeed, it is not clear how Lamarckism bears on the case at all; for that is an hypothesis to explain the origin of adaptations only; and fails, equally with natural selection, to explain non-utilitarian characters, including the mutual sterility between species.

There may be special cases, in which the direct action of external agencies has been the prominent factor in the formation of a new species; but certainly the failure of natural selection to explain all the phenomena of evolution does not oblige us to believe in Lamarckism, either wholly or in part.

PHYSIOLOGICAL SELECTION

I have not used Dr Romanes' term "physiological selection," because, except in the Bacteria, species are not founded on physiological characters; and, therefore, no amount of physiological selection could give rise to a new species; it can at most only produce isolation of some individuals from others. The best case of physiological selection is what the Rev. G. Henslow has called "Constitutional Selection," which is a struggle for existence in which the sick and weakly give way to the healthy and strong. His illustrations are taken from thickly-growing seedlings of the same species of plant; and it is evident that this would not differentiate two morphological varieties; Dr Romanes himself did not suppose that his physiological selection could directly originate species. He always described it as a form of isolation; as also does Professor Lloyd Morgan in his "Animal Life and Intelligence." Under these circumstances I think it will

avoid ambiguity if the "physiological selection" of Dr Romanes is included as a form of physiological isolation; and I have called it "progressive infertility."

SOCIAL EVOLUTION

The principle of Selection was divided by Mr Darwin into natural selection and selection by man; and this latter he subdivided into methodical and unconscious, or unmethodical, selection. Methodical selection is when a breeder or fancier selects with care the best individuals with the intention of improving an old breed or of forming a new one. Unmethodical selection is that used by the ordinary farmer, who, without any definite attempt to form or to improve a breed, culls out the worst animals from his flock and preserves the best. The action of natural selection in human society has been ably investigated by Dr Wallace, Mr Greg, Mr Galton, Mr Herbert Spencer, Mr Bagehot, Mr Kidd, and others; and a summary of the older views will be found in Darwin's "Descent of Man," together with many reflections of his own; but, strangely enough, the action of the kindred principle of selection by man—or artificial selection, as Sir C. Lyell called it—has either been overlooked, or else has been mixed up with that of natural selection, although, in reality, the two are quite distinct. Natural selection is brought about by competition between the individuals themselves, by the co-operation, as it were, of the individuals selected; and both it and preferential selection are forms of *Intrinsic Selection*. Artificial selection, on the contrary, is the action of an individual, or a group of individuals, altogether outside the

individuals selected from; and it may therefore be called *Extrinsic Selection*. In civilised society we see the action of intrinsic selection in the various effects produced by the desire for accumulating wealth; while the elimination of criminals from society by means of imprisonment is an example of the working of extrinsic selection. Extrinsic selection plays a very important part in social evolution, much more so than in biology.

An animal obtains its food, defends itself from enemies, and is protected from the weather, by its bodily structure; and, if the surrounding conditions change, a change in bodily structure becomes necessary to adapt the animal to the new conditions. But man obtains his food and defends himself by tools and weapons; he protects himself from climate by houses, clothing, and fire; and we cannot conceive any change of conditions that could not be met more readily by new inventions than by change of bodily form. As Dr Wallace says, "Man's intellect has emancipated his body from the action of the law of natural selection;" he adapts himself mentally, not corporeally, to new conditions; and this must always have been the case, ever since his mental faculties became predominant.

But although the general form of the body of man is, and has been for a long time, almost stationary—because unacted upon by selection—this is not the case with his mind; the principle of selection has not disappeared, it has only changed its venue. Skill and ability take the place of bodily structure; and, in the struggle for existence between man and man, these constantly tend to improve; and wealth, which is the chief test of a successful struggle, again assists its

possessors by enabling them to give a superior education to their children.

In animals variations are chiefly in structure, and cannot alter much during life; consequently they are small and accumulate slowly. But in man the variations are mental; they can occur at any period of life; they may be great and sudden, and can be transmitted to large numbers at the same time; and it is this which is the cause of the greater mobility of human affairs. Also, just as abundance of bodily food is the chief cause of structural variation, so also is the abundance of mental food the chief cause of mental variation. As G. H. Lewes says, "Experience is to the mental organism what nutrition is to the bodily organism." Consequently the promulgation of ideas must be a great cause of mental variability.

In human affairs selection acts not only through the principle of utility but also through that of sympathy; these two, acting sometimes in opposition, sometimes in conjunction, make the operation of selection far more complicated than in biology, where sympathy is practically absent.

It has been sometimes supposed that sympathy is opposed to selection; but this error could only have arisen by confusing selection with its agent, utility. Selection acts solely for the benefit of the selecting power; and, when the results of sympathy are advantageous, they will be preserved by selection. Man is not like the lower animals; and an advance in morals through the action of sympathy may be more advantageous than an advance in wealth or strength through the action of utility.

In man the principle of selection has been trans-

ferred from the body to the mind, to the realm of opinions. These opinions arise—psychologists tell us—from ideas or desires, and these ideas from sensations or intuitions; but *how* ideas originate we are profoundly ignorant. We are just as profoundly ignorant of the cause or causes of variation in bodily structure; yet the principle of selection, which acts upon these structural variations when formed, is known, and explains to a very large extent how it is that plants and animals are so diversified. In the same way the principle of selection acts upon the opinions and actions of men, and it is capable of explaining most of the facts of sociology. The origin of the opinions, whether by free-will or necessity, has nothing to do with the working of the principle; the supposed best opinions are selected, no matter how they arose, and these opinions govern society for the time. This is the solution of the old paradox, that either free-will must be a delusion, or else historical science is impossible; both can exist together, because the law of history is not concerned with the origin of ideas. We do not know how an eye originated; but, being there, we know its functions, and how it will be acted upon by selection; and similarly, we may not know how an opinion arose; but, once having arisen, we can estimate its probable action and trace out its development by selection; and consequently we can frame a science of history.

The process of selection in human affairs is sometimes natural selection, but more often it is preferential selection; for it is generally due to choice, voluntarily exercised, and is not an inevitable necessity, nor is it always accompanied by the destruction of the rejected.

Isolation, although not absolutely necessary for the preservation of mental variations, is very influential in securing divergence; and it has been the chief cause of variety in customs, dress, language, etc. The isolation is due either to migration or to social segregation into classes; but the barriers are rapidly crumbling, and nations are getting more and more uniform.

The truth contained in the great-man theory of Cousin and Carlyle lies in the fact that great men have great powers of causing variations in the opinions of others, of making large numbers tend to vary in the same direction. Physical agencies, on which both Montesquieu and Buckle laid so much stress, can only act indirectly in the formation of ideas through sensations. Climate may act directly on the nervous system; or a combination of meteorological and geological agencies may determine whether any particular community shall be agricultural, pastoral, mining, manufacturing, or commercial; and these act on the formation of opinions, which then come under the principle of selection. Comte's law of the three states, so far as it is true, is to sociology what Von Baer's law—that development is from the general to the special—is to biology; the cause of both is selection.

In his very interesting book on Social Evolution Mr Benjamin Kidd points out the antagonism existing in human society between the interests of the individual, necessarily concerned with his own welfare, and the interest of the community, which is largely bound up with the welfare of posterity. He says that in a Democracy there can be no rational sanction for progress, because the interest of the masses is to abolish

competition, and to organise, on socialistic principles, the means of production and the regulation of the population, so that there may be comfortable existence for all. This, he thinks, is corrected by religious beliefs, which, he says, are ultra-rational, and which inculcate the sacrifice of the individual in the interests of generations yet unborn.

Experience, however, hardly bears out Mr Kidd's conclusions. The pure democracies belonging to the western civilisation are not so socialistic as those countries in which the masses have not yet got full power; and there is no evidence to show that this is due to the greater development of religious beliefs. The real reason lies in the inherent antagonism between the different groups, which must exist in every civilised community. Socialism among savages is possible, because all have the same interests, all follow the same avocations, and there is no money. But with division of labour different interests arise; and, as every civilised community must consist of many groups, there must be many conflicting interests, which neutralise each other and ensure the continuance of competition. It is this that secures the material progress of the community as a whole, whether it be a democracy or a despotism, and not the development of religious beliefs. The desire for liberty will always prevent a civilised democracy from going back to socialism.

THE OBJECT OF ORGANIC EVOLUTION

The ultimate aim of the study of nature is to try to discover how the various objects we see came into being, and why they are here. It may not be possible

ever to attain this aim ; but we can advance towards it ; and in doing so we learn much about the relation of the universe to its Creator.

For a long time the outlook was hazy, and it was not until the publication of Mr Darwin's " Origin of Species " that a clear view was obtained. It was then seen that all animals and plants were genetically related to each other, and that the physical evolution of the solar system had been followed by a physiological evolution. When this truth had been firmly established, the inference necessarily followed that the object of physiological evolution was the development of man. The reason for the inference was, not that man is the highest product of evolution, for other animals have in their turn been in that position ; but that the advent of man had made a complete change in the course of evolution, and had added a new form—psychical evolution—to the older forms. The changes brought about were as follows :—

First, the process of natural selection is over, so far as man's bodily structure is concerned. No important change has taken place in his body since the commencement of the Pleistocene period ; and none can take place in the future, because his hand is quite capable of carrying out the ideas evolved in his brain ; and it is by these that he lives and competes with his neighbours.

Secondly, the diversity of animal and plant life on the earth seems to have reached its maximum. Man is a destructive force in nature, and already he has exterminated several of the larger animals. As time goes on we may expect that all animals and plants that are hurtful to him will be destroyed, and that the

whole earth will become a garden, the sea alone being able to resist his encroachments.

Thirdly, there are a number of elementary substances in the world which appear to be of no use except to man: for example, gold, silver, lead, zinc, &c. These must have been intended for his use, for they were useless in the economy of nature until a sufficient amount of intelligence had been reached. Not only were these made for man, but they appear to have been made as rewards for the exercise of his intellect. There are other substances, such as the rarer elements, of which no use seems ever likely to be made, except the important one of stimulating inquiry.

As we must suppose that there is no effect without a cause, and that everything has a meaning or an object, we arrive at the conclusion that the object of physiological evolution on the earth was the development of the human mind, to which the development of his body was only a preliminary.

But the intellect of man, however noble, is not the highest part of him. This is only an exaltation of characters which he shares with the brute creation. It is in his moral and religious nature that we see the true human characteristics, which separate him completely from all other animals. On the psychical evolution of man I need not enlarge. It is partly utilitarian, partly ethical, partly religious. Ethical evolution is founded on animal instincts, such as the love of offspring; and, although to some extent the product of intellect, it is chiefly due to sympathy as opposed to selfishness, and to conscious efforts at self-improvement—to what is called the prompting

of the conscience. For even in the higher phases of ethical and religious development progress is secured by the same process of conflict by which it is secured in physiological evolution. Both material and intellectual progress are due to conflict between one individual and others; while moral and religious progress is secured by the conflict of each man with himself. This was not possible until man had obtained the power of free choice; and this, therefore, was the true beginning of ethical evolution. Ethical character is acquired after birth, and is not transmitted physiologically from parent to offspring. Each human being has to work out his own development; but he is helped in this by the thoughts of former generations. With the moral comes also the religious development. Beginning at first with very low and perhaps unworthy motives of self-advancement, it has gradually led up to an endeavour to understand and to do the Will of God.

There must be some reason for all this. We cannot believe that the universe has been called into existence merely for the amusement of its Creator; for then we should be obliged to assume that He took pleasure in seeing pain inflicted on His creatures. But if the object is the moral development of man, that difficulty vanishes; for it is the sight of pain and suffering which arouses sympathy, and forms one of the strongest motive powers in ethical progress. It would be impossible to cultivate virtue in a man who did not know what evil was; for virtue consists in resisting temptation; and the existence of evil is necessary for the psychical evolution, which has been going on ever since the advent of man.

But the goal of psychical evolution does not seem to lie in this world; and, as we cannot believe the process to be altogether useless, it follows that there must be something outside this world, which is beyond the power of our intellect to fathom, but which seems to be necessarily connected with a future existence. And, if this is the case, we have in the development of human souls the ultimate object of the universe, so far as we mortals can ever ascertain it.

The two highest truths we know—the existence of one God, and that death is but a step from one kind of life to another—were arrived at by the use of our emotional and moral faculties, with but little help from reason, long before the introduction of the scientific method. But we can now show reasons for believing in them, which will carry more conviction to the educated mind than did the old intuitions and instincts; and will, in time, bring the whole human race to one belief. This will be the outcome of the theory of evolution, a theory which, a few years ago, many people thought to be atheistical.

LECTURE I

DARWINISM

THE subject on which I have undertaken to lecture has of late years been presented under so many forms in magazines and in books that it is difficult to find anything that will be fresh to you. Some of these books view it from the standpoint of philosophy, others from that of biology, others again from that of ethics. Certainly the artistic point of view is still vacant; we have no poem describing Darwinism; and I have seen no pictures of our hairy ancestors with pointed ears. I shall not, however, attempt to occupy that position to-night, but shall content myself with taking up much more humble ground. After some preliminary remarks on the ideas formerly associated by naturalists with the terms "Species" and "Genus" —a correct notion of which is absolutely necessary for understanding what follows—I shall compare the argument of Lamarck for the development of species with that of Darwin, and I shall then try to explain what Darwinism is and what it is not; what it has done and what it cannot do; and this will give me an opportunity of expounding Darwin's views on inheritance and variation. As to the philosophical aspects of Darwinism, I must pass them over with scarcely a word, as I am not competent to give any original opinion on that side of the question. I think

I shall do more good by exposing the firm foundation on which Darwinism stands. All of you, I hope, know a good deal about Darwinism; for without that previous knowledge I very much doubt my ability to make so complicated a subject plain. All I expect to do is to help to clear up any haziness that may exist in your minds as to the limits of Darwinism; and to do even this I trust more to your intelligence than to my power of popular exposition.

IDEAS BEFORE DARWIN

Passing over the views of the ancient philosophers, which, although often ingenious, were not founded on careful observation, and consequently never prepared the way for further advance in knowledge, we find in the seventeenth century a general opinion prevailing that each kind of animal and plant had been separately created at the beginning of the world, and had since then increased and multiplied and covered the face of the earth. This popular belief is embodied by Milton in the seventh book of "Paradise Lost," in which Raphael, "the affable archangel," tells Adam how and why the world was made. At this time the word "species," although used by the ancients, had no particular meaning attached to it; but in the eighteenth century Linnæus gave precision to biological nomenclature, and defined a species as a group of animals or plants that had descended from common ancestors. "We reckon," he says, "just so many species as there were forms created in the beginning"; and this definition was almost universally accepted by naturalists. It was of course allowed that no direct proof of the proposition could be obtained, but it was assumed

that perfect fertility among the individuals, and such a close resemblance as might exist between parent and offspring, were sufficient to prove community of descent; while dissimilarity in appearance, and infertility, partial or complete, were supposed to prove that the individuals had descended from separate stocks, and, consequently, belonged to different species. It was known that in some cases very dissimilar individuals had to be included in the same species, because a series of links existed graduating one form into the other; and there were other cases in which dissimilar individuals were retained in the same species, because a series of links, although not known, was supposed to exist somewhere, or to have existed previously. The real test—fertility between the individuals—could rarely be applied; so that cases arose, and kept constantly augmenting in number, in which it was a mere matter of opinion whether two different individuals belonged to the same or to different species. It was, however, allowed that, if the test of fertility could be applied, it would be decisive; for it was not doubted but that sterility between different species was a provision intended from the beginning to prevent confusion between all kinds of plants and animals. It is now known that the assumption of sterility between individuals of different species was based on a very limited number of facts, and that it will not hold good in all cases.

The great difficulty of distinguishing between species and mere varieties of a species led a few naturalists—among whom was Erasmus Darwin, grandfather of Charles Darwin—to doubt the doctrine of *fixity* of species; but it was not until Lamarck published his *Philosophie Zoologique*, at the commencement of the

present century, that any definite idea was obtained of the rival theory of transmutation or development of species, since called Organic Evolution. Before going any further it is necessary to explain how it was that the question became one about the origin of species, and not of any of the larger groups. You are no doubt aware that, in the classification of plants and animals, two or more species are united into a genus, and that genera are again grouped into families or orders. Now the group "species" was supposed to have a precise boundary, all the individuals contained in it being, as I have already said, fertile among themselves, but infertile with any individuals outside. Dr Fleming, in his Philosophy of Zoology, published in 1822, says, "The term 'species' is universally employed to characterise a group consisting of individuals possessing the greatest number of common properties, and producing without constraint a fertile progeny." But no such definite boundary could be traced for the larger groups such as genera, &c. It was generally agreed that, while species were natural groups—*i.e.*, real groups existing in nature—genera, families and orders were artificial inventions of naturalists, designed to facilitate classification; the number of species to be included in a genus being, to a large extent, a matter of convenience only. Mr T. Vernon Wollaston, in a book "On the Variation of Species" published in 1855, has a chapter on "the generic theory," in which he says "the terms genus and species have been conjointly so long associated in our minds with the self-same things, that they have become almost part and parcel of the objects themselves; so that the student who does not sufficiently

reflect on their true signification is apt to regard them as of equal importance, or, more often perhaps than otherwise, to make the latter subservient to the former. This, however, is in reality the very reverse of what should be the case, as a moment's consideration will indeed at once convince us: for what are genera after all but dilatations, as it were, along a chain which is composed of separate, though differently shaped links? The links, or the actual independent bodies which constitute the chain, are the species; but the knobs or swellings are the groups into which those species naturally fall. It will consequently be seen from what has been said that the terms genus and species not only differ very considerably in importance, but in signification also. Whilst the former is merely suggestive of a particular position which a creature occupies in a systematic scale, the latter expresses the actual creature itself. So that while one applies to several animals of distinct natures and origins, though bound together by a certain bond of imitation, the other belongs to a single race alone, which it therefore exclusively indicates." Again, the well-known naturalist, Mr S. P. Woodward, writing in 1856, says "all the specimens or individuals which are so much alike that we may reasonably believe them to have descended from a common stock, constitute a species. It is a particular provision for preventing the blending of species that hybrids are always barren. . . . Genera are groups of species related by community of structure in all essential respects. . . . Families are groups of genera which agree in some more general characters than those which unite species into genera." I wish you to

notice that while genera and families are defined as related by community of structure only, species are defined as related by community of descent. This opinion was generally held by naturalists before the publication of Darwin's "Origin of Species."

This long and rather dry digression has been necessary in order that you may clearly see that the group "species" was not merely an outpost in the theory of special creation, which the defenders might abandon without any serious consequences, and fall back on the larger groups, but was the very citadel itself—the keep of the castle—which, if taken, necessitated complete submission. This was well understood by naturalists; and it explains the rapidity of the campaign which I am about to describe. But many people, not being naturalists, have said—"We grant that different species of the same genus have descended from common ancestors, but it does not follow that this holds good for different genera." That is, they would allow that the lion, tiger, leopard, and all the cat genus have had a common origin, but they deny that cats and dogs are in any way related. This error arose from not knowing what naturalists meant by "species," and not understanding that, if the reasons which had led naturalists to assume that each species had a distinct origin were abandoned, there was no other halting-place left, and that it was useless to take up other ground; because the same arguments which proved that the species of a genus were genetically related, told with still greater force when applied to any of the larger groups, except perhaps the very largest of all—the kingdoms and sub-kingdoms. The meaning

of the word "genus" in Greek—a race or nation with common descent—no doubt also contributed to this confusion of ideas; but the word "genus" never had among naturalists the same signification that it has in Greek.

To return now to our history. The Chevalier de Lamarck was a very celebrated naturalist, and Professor of Zoology in the *Jardin du Roi* at Paris. The first sketch of his theory is said to have been published in 1801; but the book in which it is developed bears the date of 1809; and of this I wish to give you a very short summary: taken, however, at second hand, as no copy is known to me in Christchurch. He commences by showing that species are not definitely separated by special characters, but run one into the other. Genera, orders, and even classes do the same; but these are avowedly artificial groups. Exceptions to this rule occasionally occur; but this is probably due to the connecting links not having been discovered. If we have a few individuals only before us, it is easy to establish differences; but, as our knowledge increases, the difficulties of discriminating between species increase also. Varieties of a species show exactly the same thing. Now these varieties, he says, are acknowledged to be due to climate, habits, manner of living, &c. The descendants of the original species having changed their situation, have, in consequence, become varieties. Why, he asks, should not these varieties go on changing until they become new species? He then refers to the great changes that have taken place in our cultivated plants and domesticated animals; and points out how impossible it is to say now from what species some of them have been

derived. We cannot find in nature our cabbages and lettuces: the domestic duck has even lost its power of flight. Changes in nature, he points out, take place slowly; yet we know that in physical geography such changes have taken place; and these alterations in physical geography must have affected the climate and the local surroundings of the plants and animals. Their habits must, therefore, have changed; and this would require that organs which were formerly not much used should be more used, and consequently they would develop more. Other organs would be less used, and would be impoverished and ultimately would become rudimentary, or even be lost altogether. Consequently it is the habit that has shaped the organism. A duck was not made web-footed to enable it to swim, but it became web-footed because new wants attracted it to the water; by repeated stretching of the toes the skin acquired a habit of extension, and in time became a broad membrane. Among plants, changes in nutrition, light, moisture, &c., caused an analogous use or disuse of their parts. In addition to use and disuse of organs, Lamarck accounted for variations by an inherited tendency to vary; he talked of "the efforts of internal sentiment," of "the influence of subtle fluids," and of "acts of organisation," which caused a tendency to progressive advancement; and this, combined with the force of external circumstances, was, he thought, sufficient to explain the transmutation of one species into another.

Lamarck's theory was altogether opposed to the teaching of the older naturalists; and, although supported by a few, was violently attacked by Cuvier and his school. Although founded on a true induction, it

gave no rational explanation of the change of one species into another, and showed no efficient cause for gradual divergence of characters. The adaptive principle, although true, was made to do more work than it could bear, and the mixture of metaphysics discredited the whole. It so happened that, just before his book was published, the naturalists attached to Napoleon's army of Egypt had sent to Paris a number of mummies, including monkeys, dogs, cats, ibises, birds of prey, and crocodiles. These were certainly two or three thousand years old; and no difference could be detected between them and the same species now living in Egypt; while the present fauna and flora of the earth was supposed to date back not more than six thousand years. If during the last two thousand years no change had been brought about, how, it was triumphantly asked, could such immense changes have taken place in six thousand years? Lamarck explained that the climate of Egypt had probably remained the same for more than two thousand years; and he protested against the supposed universal catastrophe by which all life had been destroyed six thousand years ago. But his explanation was in vain, for it was in advance of the age.

The idea that the present surface of the earth, with its fauna and flora, was not very ancient, was founded partly on Biblical chronology, and partly on geological reasoning. It was said by geologists that if the earth had been old, all lake basins would have been filled up; that sand-dunes would have covered much more country; that the deltas of the Nile, the Po and the Rhine would have been much larger; that the Red Sea would have been filled up by coral; while on the

other hand Biblical chronologists had made different estimates of the age of the earth, varying from 3483 to 6984 years. This seemed to strengthen the opinion of James Ussher, Archbishop of Armagh, who in the first half of the seventeenth century had arrived, on astronomical grounds, at the conclusion that the world had been created at six o'clock on the morning of the 11th of February, 4003 B.C.; which date Dr Lloyd, Bishop of Worcester, had added as a marginal note to a folio edition of the Bible published in 1701. The coincidence of these three lines of argument naturally seemed conclusive; but it may be as well to mention that Archbishop Ussher's idea was founded upon calculations, made by astronomers, that at the time mentioned the line of equinoxes coincided with the major axis of the earth's orbit, and consequently at that time winter and summer were of equal duration all over the world. This, he thought, must necessarily have been the case at the creation of the earth.

The next vigorous onslaught on the doctrine of the fixity of species was made in 1844 by the "Vestiges of Creation," the author of which book was Mr Robert Chambers. It was written in a pleasant style, and gave a popular account of the evidence in favour of the development of species; but, although disavowing Lamarck as having "scarcely a single adherent," did not advance a step beyond him. The book also contained many mistakes, especially as to the spontaneous generation of living organisms — even insects — by means of voltaic electricity; and the evident carelessness of the recorded experiments deprived the book of all scientific value. It was widely read, but had no

influence on naturalists. That Lamarck had "scarcely a single adherent" was, however, not quite correct; for ever since he wrote there have always been some naturalists who believed in the theory of development of species; and, as the facts of biology, and especially those of palæontology, increased, the disciples of this doctrine grew more and more numerous; but none could explain it.

While scientific men were thus groping in the dark after an indistinctly seen truth, the "Origin of Species" appeared, which not only gave a better account of the evidence for the development of species than had ever been given before, but showed the fallacy of the adverse argument from hybridism, and, above all, gave a lucid explanation of how progress had been brought about by means of natural selection. It was met by a storm of abuse and opprobrium, such as we now can hardly realise, and which it would be unwise to recall. Nevertheless, the effect of the book was magical. In ten years almost all naturalists were converted; in twenty years the doctrine had spread far beyond natural science into the dominions of ethics and psychology; and, a little more than twenty-two years after the publication of his book, Darwin was buried in Westminster Abbey, near the tomb of Sir Isaac Newton, literary men and theologians uniting with philosophers and naturalists to do honour to the memory of one of the greatest observers and thinkers that the world has ever seen.

So rapid has been the victory that I myself have seen the whole of the fight; and I trust, therefore, that a personal reminiscence will not be deemed egotistical.

I was brought up in the old orthodox creed of

special creation. I can well remember the time when naturalists discussed seriously such questions as "Why the creative power had been more active in Madagascar and in Madeira than in Asia and Europe?" "Why ducks had been created with hind toes which they could not use?" "Why whales had five fingers inside their flippers?" and many other curious problems. The scales have long since fallen from our eyes; and for this we thank Charles Darwin. When the "Origin of Species" first came out, I read it with avidity. I could detect no flaw in the argument; but thought that this must be owing to my ignorance. Soon afterwards I happened to make a geological excursion to the Isle of Wight with Sir Andrew Ramsay, then Director of the Geological Survey of Great Britain. Of course we talked in the evenings about the book; and I was surprised to find that he entirely agreed with its views. I had then, as I still have, the greatest admiration for Professor Ramsay. I looked up to him as a model of all that a geologist should be, and was astonished at his knowledge of detail in everything relating to his science. Judge therefore of the shock I felt when my cherished ideas were gently but completely taken to pieces and their hollowness exposed.

Needless to say I returned from that expedition a Darwinian; and during the next six months I carefully studied the subject. About that time another geological friend, the late S. P. Mackie, editor of the *Geologist*, asked me to write something for his magazine; and I, brimful of youthful enthusiam, offered an article on Darwin's book. This appeared in March and April, 1861; and, although poor enough stuff, as I now well know, it brought me an extremely kind and character-

istic letter from the great Darwin himself; a letter which, I need scarcely say, I have treasured ever since. You will not, I hope, accuse me of vanity if I read this letter to you. I wish to do so, not on account of its reference to my article, but because any letter from Darwin relating to his now famous theory must be of interest; and especially a letter written not much more than a year after his book had appeared. The letter ran as follows:—

April 20th (1861).

DOWN, BROMLEY, KENT.

DEAR SIR,—I hope that you will permit me to thank you for sending me a copy of your paper in the *Geologist*, and at the same time to express my opinion that you have done the subject a real service by the highly original, striking, and condensed manner with which you have put the case. I am actually weary of telling people that I do not pretend to adduce direct evidence of one species changing into another, but that I believe that this view in the main is correct, because so many phenomena can be thus grouped together and explained.

But it is generally of no use; I cannot make persons see this. I generally throw in their teeth the universally admitted theory of the undulations of light—neither the undulations, nor the very existence of Ether being proved—yet admitted because the view explains so much. You are one of the very few who have seen this, and have now put it most forcibly and clearly. I am much pleased to see how carefully you have read my book, and what is far more important, reflected on so many points with an independent spirit. As I am deeply interested in the subject (and I hope not exclusively under a personal point of view), I could not resist venturing to thank you for the right good service which you have done.—Pray believe me, dear sir,

Yours faithfully and obliged,

CHARLES DARWIN.

This letter is interesting for three reasons. In the first place it shows well the caution of Darwin, who

never pretended to have proved anything beyond what the evidence warranted. In the second place it shows his kindness and courtesy to young naturalists, who, like myself, were quite unknown to him. It is astonishing that he, in his mature judgment, should speak in terms of encouragement about a juvenile essay, which I should now regret having published, if it had not been the means of procuring me the letter I have just read to you. And in the third place it shows how strong, at the time, the current of popular opinion must have been against him, when he so much appreciated the sympathy of an unknown naturalist. How rapid has been the change! Let us try to find the reason for this by comparing the argument which failed with Lamarck with that which succeeded in the hand of Darwin.

DARWIN'S ARGUMENT COMPARED WITH THAT OF LAMARCK

Lamarck commenced by an induction from the facts of classification, and came to the conclusion that species do not differ essentially from varieties. So far he was right; but he then goes on to assume, without any reasons, that varieties are due to climate; which is not a legitimate induction from anything we know, as I shall show later on. He then argues that, as changes in physical geography take place slowly, therefore climate, and with it the habits of the plants and animals, must also slowly change, and, consequently, they will pass through the stage of varieties into that of new species. The only rational explanation he gives for these changes is the use or disuse of organs; an explanation quite inadequate. The argument as to

the cause of change was a failure. He gave no explanation of the extinction of intermediate forms; and his induction, that species were of a similar nature to varieties, was founded on far too limited a range of facts to carry, by itself, conviction of its truth.

Darwin sets to work very differently. He prepares the way by giving an account of the facts of variation in plants and animals under domestication, and shows how these variations have been gradually accumulated, generation after generation, by the artificial selection of man, partly in a methodical way and partly unmethodical, until the most dissimilar varieties have been produced from the same stock. These varieties of the same species, he points out, differ from each other in much the same manner as do the species of a genus in a state of nature. He then proceeds to give the facts of variation in a natural state, and comes to the same conclusion that Lamarck had arrived at before him, that species resemble varieties in many respects. But he does not stop here; he proceeds to put this conclusion to the proof, by making a deduction which can be verified. He says, "If it be true that species resemble varieties, then the species of the larger genera ought to be more variable (that is, ought to contain more varieties) than the species belonging to smaller genera." To test this, he arranged the plants of twelve countries, and the beetles of two districts, into two nearly equal masses, the species of the larger genera on one side and those of the smaller genera on the other; and in every case it turned out that a larger proportion of the species on the side of the larger genera presented more varieties than those on the side of the smaller genera; thus verifying his deduction.

Having thus laid a firm foundation, he next offers an explanation of how varieties change into species. He shows that plants and animals tend to increase very rapidly, while the amount of vegetation (and consequently the amount of animal life also), which the earth can support, is strictly limited; and so proves that more organisms come into existence than can possibly live; that, in fact, in each year as many must die as are born; and this gives rise to a struggle for existence.

Putting now together the fact of variation and the fact of the struggle for existence, he shows that generally those varieties which are best adapted to succeed in life will live the longest, and will leave most offspring, while the others will be killed off. The successful individuals will hand down their useful characters to their offspring, occasionally in a still more useful form; and thus useful variations will tend more and more to improve, and succeeding generations will diverge more and more from the original stock. This process he calls natural selection; and he shows how by its means the classification of all organic beings can be explained, and how it would necessarily tend towards a general advance of organisation; although lowly forms would still survive to occupy their appropriate places in the scheme of nature, while some might even retrograde.

He then examines the laws of variation, of which he acknowledges our ignorance to be profound—discusses how much may be due to change in external conditions, and shows how use of one organ and disuse of another may bring about rudimentary structures, by the unused parts becoming atrophied from want of sufficient nourishment.

This concludes the first part of his argument; and, before advancing, he proceeds to discuss the objections —including that of hybridism—that may be urged against the theory, and says that some of them are so serious that he cannot reflect on them without in some degree being staggered; but that, to the best of his judgment, the greater number are only apparent, while those that are real are not, he thinks, fatal to the theory. In this part of the book very nearly every objection that has since been made was considered in a thoroughly impartial manner; everything that could be urged against the theory being stated most forcibly; and its transparent honesty did as much perhaps as the previous chapters to convert naturalists to the new faith; for it showed that, after all, the argument for the immutability of species was a very weak one. And, year by year, the difficulties here discussed have grown fewer, while the argument in favour of the theory of development remains incontrovertible.

Having thus proved natural selection to be a true cause, and one that largely explained the origin of species from varieties by causing a gradual divergence of character, and having considered all the objections to that theory, he next proceeds to push the argument further, by passing in review all the facts of geology and biology that bear upon the subject. He treats on the geological succession of organic beings, on the geographical distribution of plants and animals, on the mutual affinities between organic beings, on morphology and homologies, on embryology, and on rudimentary organs. He shows how the theory of descent offers an easy explanation of nearly all the phenomena of organic nature; and so accumulates

induction on induction as to leave no loop-hole for escape.

In a concluding chapter he sums up the results, and points out that the chief cause of our unwillingness to admit that one species has given birth to other and distinct species is, that we are always slow in admitting great changes of which we do not see the steps. The mind cannot possibly grasp the full meaning of the term of even a million of years; it cannot add up and perceive the full effects of the many slight variations accumulated during an almost infinite number of generations; and he ends as follows—"It is interesting to contemplate a tangled bank, clothed with many plants of many kinds, with birds singing in the bushes, with various insects flitting about, and with worms crawling through the damp earth; and to reflect that these elaborately constructed forms, so different from each other, and dependent on each other in so complex a manner, have all been produced by laws acting around us. From the war of nature, from famine and death, the most exalted object which we are capable of conceiving, namely, the production of the higher animals, directly follows. There is a grandeur in the view of life, with its several powers, having been originally breathed by the Creator into a few forms, or into one; and that, whilst this planet has gone cycling on according to the fixed law of gravity, from so simple a beginning endless forms, most beautiful and most wonderful, have been and are being evolved."

Who could resist such an encyclopædia of knowledge? What was there to oppose it? Nothing but empty rhetoric, and a *réchauffé* of the difficulties already considered by Darwin. It was seen that the

only argument which could be urged in favour of the special creation theory was the old one of the sterility of hybrids; and even that had broken down before the records of the numerous zoological and botanical gardens. Nothing could be done except to cover the retreat by raising a cloud of futile objections; and these soon ceased to have any weight. The victory was complete, and Darwinism has reigned ever since.

In 1871 the "Descent of Man" was published, but met with little opposition, as it was an anticipated corollary from the theory of descent. As soon as it was shown that, in his bodily structure and in his embryonic development, man resembled the lower animals, and especially when it was shown that man had rudimentary muscles, which were useless to him, but which were well developed and useful in other mammals, it was seen that he could not be excluded from the theory. Difficulties were raised against natural selection having been the sole cause of the descent of man from the Apes, and these difficulties have not yet been overcome. At the present day no one can explain the origin of man; but all are agreed that he is descended from the lower animals.[1]

It is generally thought that Darwin owed the idea of natural selection to Malthus. Indeed, he seems to have thought so himself; for in a letter to Professor Haeckel he says, "With my mind thus prepared I fortunately happened to read Malthus's 'Essay on Population,' and the idea of natural selection through the struggle for existence at once occurred to me." But Darwin forgets for the moment that, long before

[1] It is now generally agreed that the origin of man was due to selection and isolation (1899).

this, he had stated the Malthusian doctrine more correctly than Malthus himself; for in his "Journal of a Naturalist" he says, "We do not steadily bear in mind how profoundly ignorant we are of the conditions of existence of every animal; nor do we always remember that some check is constantly preventing the too rapid increase of every organised being left in a state of nature. The supply of food on an average *remains constant;* yet the tendency in every animal to increase by propagation is geometrical; and its surprising effects have nowhere been more astonishingly shown than in the case of the European animals run wild during the last few centuries in America. Every animal in a state of nature regularly breeds; yet in a species long established any great increase in numbers is obviously impossible, and must be checked by some means." Here we have the germ of the theory of the struggle for existence stated correctly, and exactly as it was re-stated twenty years later. Malthus, on the contrary, says that, while man tends to increase in geometrical ratio, his means of subsistence increases in *arithmetical ratio* only; a statement difficult to prove, and different from that of Darwin. It is quite possible that the reading of Malthus's book may have blown into flame the spark already existing in Darwin's mind, and so may have been the means of evolving the theory of natural selection; but the whole credit really belongs to Darwin; and it was only his excessive modesty that made him push the name of Malthus to the front.

SCOPE AND RESULTS OF DARWINISM

The essential feature of Darwinism is, undoubtedly, the principle of selection; but Darwin did so much to

further the acceptance of the theory of descent, that the whole doctrine of modern evolution is popularly associated with his name. This, however, is not correct; and Darwin himself would, I think, only claim that he had made the theory of development of species credible, and had thus indirectly influenced other subjects than biology. Darwin never used the word evolution until long after the "Origin of Species" was published. With him, as with all naturalists, it was always called the development of species. The modern idea of evolution came about in this way.

In 1852 Sir W. Thomson (now Lord Kelvin) discovered the principle of the dissipation of energy; which is that, owing to the radiation of heat into space, where it is lost, the energy of the solar system is gradually, but surely, being dissipated; and this dissipation of energy, added to the nebular hypothesis of Laplace, gave rise to a theory of the progress or evolution of the solar system.[1] It was certain metaphysicians who connected this physical evolution with

[1] Notwithstanding the fact that, so far as we know, the whole stellar universe is composed of the same elementary substances, we have no reason to suppose that it was formed by one operation. On the contrary, it seems to be a collection of independent stars, not arranged with any regularity, moving in all directions, and occasionally colliding. Consequently we cannot suppose that these stars owe their origin to any single scheme of development. Nevertheless the energy in the universe, which is now very unequally distributed, must slowly but surely be tending towards equal diffusion.

Some astronomers think that the stellar universe is limited in space, and that there are no stars so far off that we cannot see them. Bright stars, faint stars and nebulæ may all be at approximately the same distance from us, and may differ only in brightness.

If this be the case, there must be a slow concentration of stars towards a centre; and a time will come when all the stars will coalesce into a gigantic mass, which will cool down to the temperature of space.

the development of species, and made one great doctrine of the evolution of the universe, including the hypothesis of abiogenesis, or the spontaneous generation of living

If therefore the universe is limited in space, it must also be limited in time. For if the beginning had been an infinitely long time ago, this process of concentration and cooling would be over. Many astronomers, however, see no proof of the universe being limited in space, and consequently no proof that it is limited in time; and it is possible that the unequal distribution of energy may be maintained by the occasional addition of new stars.

The case is very different with the solar system; for here the regularity of movements, the gradual increase in density of the planets the nearer they are to the sun, and the uniformity of chemical composition, distinctly imply a common origin. We also know that the sun is radiating away large quantities of heat each year; and yet its temperature cannot have been much greater than it is now ever since life appeared on the earth, for the heat of the ocean depends largely on the heat of the sun. The sun, therefore, must be receiving accessions of heat, which can only be due, either to a constant rain of meteorites on to its surface, or to concentration of its mass. It is hardly possible that the first could supply sufficient heat, for meteors falling on the earth cause no appreciable rise in temperature; and therefore a large part, if not the whole, of this heat must be due to condensation, that is to shrinking. Consequently the sun must formerly have been larger, and probably cooler, than it is now; and we can easily imagine that originally it embraced the whole solar system.

When we examine our own planet, we find abundant evidence of a geological evolution. We know that at one time the earth was a molten globe, the exterior of which slowly cooled and consolidated. When sufficiently cold, the water, which had previously existed as vapour in the atmosphere, condensed into rain, which fell on the earth, filled up the hollows in the crust and formed the ocean, on the surface of which the first living organisms appeared. With the first rain, denudation of the land and the formation of sedimentary rocks commenced; and the volume of these rocks has continually increased up to the present day. Raised from the bed of the ocean by the movements of the heated interior of the earth, they now form the greater part of the continents, while at the same time the oceans have probably increased in depth. This will go on until the interior of the earth is no longer sufficiently hot to cause oscillations of its surface; and then all the land will be gradually swept into the sea.

matter. But they attempted no proof of this connection. Indeed, none can be given.

But, although Darwinism and the modern doctrine of evolution are two very different things, it is undoubtedly true that, but for Darwin, we should not have heard much about evolution; for it was he who taught us to believe in what is now called organic evolution, an essential part of the larger doctrine, and the one on which the greatest stress is laid by evolutionists. But, leaving on one side this supposed connection of organic with physical evolution—a connection never agreed to by Darwin—let us see what were the direct effects of the acceptance of the theory of the development of species.

In Biology it at once revolutionised our ideas about classification. When I was young I was taught that there were two different methods of classification, called respectively artificial and natural; that there might be many different classifications under either of these heads; and that it was to a large extent a mere matter of individual opinion as to which was the best; each and all having for its aim "the double purpose of generalising acquired facts, and of facilitating the diffusion of biological knowledge, by rendering it possible to recognise plants and animals which have been already described" (Henfrey). The artificial methods had more especially as their object an easy means for finding out the name of each species; and, with this view, some one character was taken to separate the groups, another the sub-groups, and so on. A familiar example is the classification of plants made by Linnæus. He first divided all flowering plants with perfect flowers into twenty classes, according to the

number and coherence of the stamens; and each of these classes he again divided into orders, according to the number of carpels. This was very simple and easy to understand; but unfortunately it brought together plants which were evidently very unlike, and it often separated plants which were evidently closely related. Indeed, there are cases where, if the system were logically carried out, it would place in different classes different flowers on the same plant. The natural methods aimed more at bringing together plants or animals which their general structure showed to be mutually connected; and they took a combination of characters as a test of this relationship. These classifications were intended to point out the agreements among organisms; and they were only indirectly useful as a means of recognising the name of a species. The great difficulty here was to find out which characters were of more importance, and which were subordinate; and this naturally gave rise to very different opinions, so that the natural systems became as numerous as the artificial ones.

All was in confusion until the "Origin of Species" appeared, in which Darwin pointed out that there was only one natural system of classification; that which represented a genealogical tree. The terms "affinity" and "relationship," so long used in a metaphorical sense, had now a plain meaning; naturalists were no longer "incessantly haunted by the shadowy doubt whether this or that form be a true species," and all had the single object in view, of tracing out this genealogical tree. But Darwinism did not stop here: it gave naturalists three entirely new clues wherewith to trace out genealogies. One is embryology, which,

as Darwin says, will often reveal to us the structure of the prototype ; the second is the study of homologies, or similarities of structure in organs fulfilling different functions ; the third is reversion, or atavism, by which long lost characters are occasionally reproduced. Indeed, one of the most important discoveries of Darwin —one that will bear fruit in years to come—is the remarkable phenomenon of latent characters ; a subject to which I shall refer presently.

Again, how much have biologists learnt from Darwin's experiments and observations on the effects of cross-fertilisation in plants. The discovery of the reason why there are two sexes in plants and in animals, is of the highest value in physiology ; while his observations on sexual selection among animals, and on the means of securing cross-fertilisation among plants, have clearly proved that a large number of elaborately formed organs are of adaptive origin ; that is, they have been gradually formed because they proved to be useful, and were not simply used because they were there. Once again, how plainly is now the meaning of rudimentary organs in plants and in animals ; how often we can read their history, and see dimly the past stages in which these organs were once useful to their possessors. How true now becomes the pregnant sentence of the master, " The structure of each part of each species, for whatever purpose used, will be the sum of the many inherited changes through which that species has passed during its successive adaptations to changed habits and conditions of life." Truly, " we no longer look at an organic being as a savage looks at a ship, as something wholly beyond his comprehension ; " and this is the result of Darwinism.

Turning now to Geology, we find three great results from the teaching of Darwin. First, we have had our eyes opened to the imperfection of the geological record. Dimly seen at first, as the necessary effect of the destruction of rocks by the denudation of sea and rain, its full significance has now been forced upon geologists with a hand of iron; and this has had the effect of stopping many a wild speculation about universal catastrophes and the replenishment of the world. Secondly, by means of the theory of descent geologists are beginning, as Darwin said they would, to gauge with some security the relative duration of intervals of time by a comparison of the preceding with the succeeding forms of life; or, in other words, we have learnt that the amount of the agreement or disagreement between two successive fossil faunas depends largely upon the length of time which separates them. And thirdly, by proving that the species of a genus, and the genera of a family, have all descended from common parents, a flood of light has been thrown on the former physical geography of the earth. By this means we can prove that Madagascar was once united to Africa, Borneo to Asia, Tasmania and New Guinea to Australia; that New Zealand has been separated from Australia ever since the cretaceous period; and that it has since then always remained as an island, and has never been entirely submerged below the sea. This, and much more, which I have no time even to mention, we could not have proved except for Darwinism. Indeed, by means of Darwinism, geologists can prove that there has always been land and fresh water on the face of the earth ever since the Silurian period. That

ever since the Cambrian period there has never been a collision of the earth, or sun, or any member of the solar system, with a comet, or any other heavenly body, by which heat, sufficient to destroy life on the earth, has been generated. And if, knowing that the earth has supported life continuously for more than twenty millions of years, we can now, by the theory of chances, look forward trustfully to the future, and deride the prophets, who foretell direful collisions with comets by which the earth shall melt away, we have to thank Darwinism for our new confidence.

There is another evil dream from which we have been relieved by Darwinism. I allude to the idea so well expressed in "In Memoriam" that Nature, red in tooth and claw with rapine, shrieked against the creed of the goodness of God as displayed in Nature.

> Are God and Nature then at strife,
> That Nature lends such evil dreams?
> So careful of the type she seems,
> So careless of the single life;
> . . . . .
> 'So careful of the type?' but no,
> From scarped cliff and quarried stone
> She cries 'A thousand types are gone;
> I care for nothing, all shall go.'

Darwin has taught us that "I care for nothing, all shall go," is a mistake. It is doubtless true that the individual must perish; but it perishes for the good of the race; or perhaps for the good of some other race; and indirectly, it is by the death of the individual that progress is secured. It is only by narrowness of vision—by fixing our eyes on the individual instead of on the race—that we get the idea that God and

Nature are at strife. So far as man is concerned we have every reason to believe that he will never become extinct, but that the form and proportions of his body, which at present distinguish him, will remain essentially the same to the end; because by the development of his intellect his body has become liberated from the law of natural selection.

The effects of Darwinism on Psychology and Ethics have also been very great; but they are chiefly due to the development of the doctrine by others. Mr Darwin, however, clearly saw the line his views would lead to; for in the "Origin of Species" he says—"In the future I see open fields for far more important researches. Psychology will be securely based on a new foundation, that of the necessary acquirement of each mental power and capacity by gradation."[1] And, though not a trained psychologist, his researches have gone far towards revolutionising that branch of enquiry. The chapter on Instinct in the "Origin of Species" was his first contribution in this direction; and it cleared up much ambiguity by showing that, although some instincts may be inherited habits, as taught by Lamarck, others have certainly been acquired unconsciously by means of natural selection. But of far more importance were the chapters in the "Descent of Man" on the comparison of the mental powers of man with those of the lower animals, and on the development of the intellectual and moral faculties during primeval and civilised times. Darwin himself modestly says, "This great question (the origin of the moral sense) has been discussed by many writers of consummate

[1] This "new foundation" had been already laid down by Mr Herbert Spencer.

ability; and my only excuse for touching upon it is the impossibility of here passing it over; and because, so far as I know, no one has approached it exclusively from the side of natural history." Nevertheless, in the opinion of Mr Romanes, no mean judge, the result of this investigation has been "to give, if not a new point of departure to the science of ethics, at least a completely new conception as to the origin of the faculties with which that science has to deal;" and, according to Professor Clifford, it is "the simplest and clearest and most profound philosophy that was ever written upon the subject."

LIMITATIONS OF DARWINISM.

Such is Darwinism, and such are some of the effects that have flowed from it. How far they will extend in the future no one can say. Nevertheless, Darwinism has its limitations; and I will now proceed to point them out.

In the first place, natural selection does not appear to be capable of explaining all the changes that have taken place in plants and animals, but will only account for a certain number of them. This was always recognised by Darwin; and in the Introduction to the first edition of the "Origin of Species" he says distinctly, "I am convinced that natural selection has been the main but not exclusive means of modification;" and this is repeated in all subsequent editions. Many biologists are of opinion that there is an "inherent tendency" towards higher organisation; they think that protoplasm tends to become more and more complex, and that evolution is the inevitable outcome of a fundamental property

of living matter. This view is due to the difficulty of explaining some phenomena in any other way; and it may be taken merely as an expression of our ignorance, whether or no it be true. As a matter of fact, natural selection is at present the only known efficient cause of progress; for the "use and disuse" hypothesis is as yet unproved.

Again, natural selection does not account for the origin of varieties. Darwin starts with the fact of variation, and shows how natural selection will affect it. On this account the name, "Origin of Species" has sometimes been objected to; but without any reason. If the book had been called "The Origin of Species by the Natural Selection of Varieties," all ambiguity on this head would have been avoided. The origin of varieties and the laws of inheritance are not touched upon by the theory; and, although Darwin deeply investigated these mysteries, and proposed a provisional hypothesis, called Pangenesis, to group together the facts, it forms no part of Darwinism; and it is understood that Darwin himself abandoned this hypothesis, or thought lightly of it. Still, it will conduce to the understanding of the limits of Darwinism if I briefly describe Pangenesis, together with some of the known facts of inheritance and of variation; and, as nearly all the facts have been collected by Darwin, the subject is strictly Darwinism, although not included in the popular sense of the word.

PANGENESIS

To take the subject of inheritance first: starting from the observed fact that a particle of living

protoplasm grows by the assimilation of food, we can understand that this living particle, endowed as it is with the power of contraction, might, when it reached a certain size, divide into two, and thus form two individuals exactly alike, and so on *ad infinitum*, so long as the supply of food lasted. But what are we to think when we find that the two portions of protoplasm do not separate, but remain attached, and that each grows by numerous divisions into quite different organs? What further are we to think when we find that a portion of protoplasm, detached from this organism, will go through a life-history exactly similar to that of its parent? Take for example the particle of protoplasm called the oöspore of a fern. This will divide into two parts, by the formation of a thin membrane of cellulose; and afterwards these two cells will develop quite differently, one forming the stem and fronds, the other the root. The root will perish altogether, but the fronds will produce spores, each one of which on germination will produce a little plant, which in its turn will produce oöspores; and each of these oöspores will go through exactly the same development as the first one did.

No one as yet has been able to frame any probable explanation of these facts; for Mr Darwin never supposed that his hypothesis of Pangenesis was more than a speculation, which grouped together a large number of them. This hypothesis assumes that each cell of an organism gives off minute detached particles, called gemmules, which are capable of multiplying by self-division. These gemmules circulate through the whole system, and are collected together into those cells whose function it is to produce a new organism. In

this way they are transmitted by the parent to the offspring; and each gemmule is then capable of developing into a cell similar to that from which it was detached. But it can only develop when it finds itself in a position in the new organism similar to the position of its parent cell in the old organism. For instance, suppose that in a growing plant the cell A produced by division the cell B and that each gave off gemmules called α and β respectively. Then in the young plant β can only develop on α in the same position that B held to A; and, until it finds that position, β may be transmitted from generation to generation in an undeveloped state; but it will always be capable of development whenever it finds the necessary conditions.

It is evident that this hypothesis does not account for the first origination of the ancestor of B; it only accounts for transmission from one generation to another. Still, so far as transmission is concerned, the hypothesis might go far towards explaining the facts of inheritance, if these facts were always constant. But such is not the case. In the first place, the force —if the expression may be allowed—the force of inheritance, although always strong, varies in different races, or even in different individuals of the same race. For instance, most people know that, although some lop-eared rabbits breed true, in others the tendency to produce young with drooping ears is very uncertain; and this is not due to any impurity of blood; for brothers and sisters differ much in this respect. It is not so well known that if the seeds of a "weeping" tree—that is a tree in which the branches hang down —be sown, some of the young plants will show a tendency to weep at once; others perhaps not for twenty

years; others not at all. This proves incontestably that the power of transmission is individual, because it varies in different individuals; so that, if these characters are due to the development of inherited gemmules, different individuals must contain different quantities of these gemmules, and consequently their number must be limited.

On the other hand, one of the most remarkable facts connected with inheritance is, that transmitted characters may remain dormant, or latent, in one individual, who nevertheless may pass them on to another individual in which they may be developed. A good milking cow, for instance, can transmit her good qualities through her male offspring. Sometimes, indeed, these latent characters may be abnormally developed; as when an old hen assumes the plumage and spurs of a cock and begins to crow; but usually they remain latent all through life. Indeed, ancestral characters can remain latent for many generations. For example, all our various breeds of fancy pigeons are undoubtedly descended from the wild "Blue Rock," which is chiefly characterised by its blue colour, and by having two black bands on each wing, and one on the tail. Everyone knows that some of the races of fancy pigeons have none of these characters. Some are pure white; some pure black; and several races are known to have bred true for more than a century. Yet Mr Darwin has shown that, if any two breeds of pigeons are crossed, no matter what their colour may be, the young almost always, and in the second cross always, show a tendency to become blue, and to have two black bars on the wings, and one on the tail; in fact, to revert to their ancestral characters, which must,

in many cases, have lain dormant for an unknown number of generations.

Now, if these characters result from the development of material bodies, or gemmules, given off by their remote ancestors, the gemmules must either be given off in numbers altogether incredible, or they must be capable of multiplying *ad infinitum*. But, on the latter supposition, how could the power of transmission vary in different individuals? and how could one individual inherit a character more strongly than another, as in the case just mentioned of weeping trees? If the gemmules are capable of multiplying, it is incredible that the transmission of a few more or less could make any difference. But if gemmules, either in finite or in infinite numbers, or in finite numbers but capable of infinite increase, are inadmissible, in what way can we conceive any material particles to exist? And yet, without some special material body to work upon, how can we conceive any force reproducing latent characters at all? Here then we have a mystery lying altogether beyond the boundary of Darwinism, and one which Darwin never pretended to have explained.

It is much the same with the facts of variation. Here also the great difficulty in framing an hypothesis that will account for the facts is the great irregularity among the facts themselves. But, before giving you illustrations of this, it is necessary to remind you that offspring may differ from their parents in two ways, first by developing new characters, and secondly by reversion, or the development of latent ancestral characters. Now by variation naturalists mean the first only; but it is often not easy to decide whether

a difference between parent and offspring is due to variation or to reversion.

The tendency to vary differs very much in different species of plants and animals; in some it is difficult to find two individuals alike, in others it is difficult to find any difference, even when a considerable number of individuals are compared. Groups of species sometimes follow the same rule; for example, the veronicas in New Zealand are among the most variable of plants, while the veronicas of Europe are remarkably stable. But more important still is the fact that the tendency to vary can be modified by man. Horticulturists, when they want to prevent plants from varying, sow the seed on poor soil; if, on the contrary, they wish for variations, the plants are grown on good land, and separated from each other, so that they may grow vigorously. It often takes time to make a species vary. Wild flowers on first being cultivated do not vary for several years; and wild ducks when kept tame do not vary for the first five or six generations; but when in time the original colour or form shows an alteration, then variations will be abundant, and generally in many directions. Every one knows with what rapidity new varieties of flowers are introduced by gardeners, when once the strict rule of inheritance has been broken through. Evidently the tendency to vary is increased by cultivation or domestication; and it seems connected with an excess of food. But here again we come across many anomalies; in some species domestication does not induce variation at all, as in the goose or in the rye, both of which hardly vary, notwithstanding that they have been domesticated for ages. Again, the

variations induced by excess of food are not always accompanied by increase in size; for we have small varieties of dogs and of fowls produced by the same means as the large varieties.

Some parts are much more liable to vary than others. Colour is generally more variable than form; but the opposite may be the case in some species. Again, variation in one part is sometimes accompanied by variation in another part: for example, in pigeons the beak and feet vary together. Horses with a white star on the forehead generally have white feet; while in black-and-tan dogs tan feet go with tan spots over the eyes.

It would naturally be thought that changes in external conditions, especially in climate, would produce variation in the plants and animals; and this was the view which Lamarck took. But a little investigation shows that such is not the case. No doubt, any particular individual may be affected by climate, as we see with trees grown in the wind or in the shade; but we have every reason to think that these individual variations, caused directly by the action of climate, are but rarely, if ever, transmitted to the offspring, and do not therefore accumulate. A familiar example is found in the sheep, the tails of which have been cut off for many generations; and yet no farmer expects to find his lambs with short tails. The fact that many species of plants and of animals are found living, almost all over the world, under the most different conditions of food and climate, without any sensible variation, is alone sufficient to show that a change in external conditions does not necessarily produce variation; while the

fact that variations may be produced under apparently exactly similar conditions (as when two varieties of flowers occur on the same plant), shows that change is not necessary for variation.

Again, variations take place under all latitudes, wherever plants and animals are found; and it is by no means necessary that the climate should be a congenial one to induce variation; for the orange, which can barely live in the north of Italy, has varied there very much. And again, nearly similar variations may occur under very dissimilar climates, as is illustrated by the remarkable analogous varieties of pigeons in India and in Europe. If we turn to domesticated animals, we find that variability is by no means closely related to the amount of change in conditions. The most variable of domesticated birds in Europe are the pigeon, the fowl, and the duck; and of these the pigeon and the duck are natives of Europe, while the fowl is a native of India. On the other hand, the goose, guinea-fowl, turkey, and peacock, have hardly varied at all in Europe; and yet the guinea-fowl comes from Africa, the turkey from America, and the peacock from India. Evidently change in external conditions will not account for variation, and individual constitution is of more importance. At the same time, domestication, with an abundant supply of food, in time induces most plants and animals to vary; and consequently, change in conditions and in habits must in some way indirectly promote variation; but whether there is in addition any innate tendency to vary, we have as yet no sufficient proof.

So complicated and apparently contradictory are

the facts of variation, that some naturalists have assumed that no law reigns at all; that, in fact, variation from the parent in every direction is the normal condition, and that inheritance is the law which checks variation. If we could accept this idea, it would get over many difficulties in the way of explaining evolution; but Darwin, ever loyal to the truth, has shown that it cannot be accepted, and that the phenomena of inheritance are quite as irregular as those of variation.

On the hypothesis of Pangenesis we might look upon variation as the occasional abnormal development of gemmules in two ways: either by gemmules developing on other cells than their proper neighbour; or else that, although developing in the right place, they do not correctly reproduce the parent cell. In the first case we must suppose that the gemmule, which ought to have developed in that place, was absent; and consequently, that a variable species is one that produces few gemmules. This, of course, implies that different species give off different quantities of gemmules; and consequently, that their number is not infinite; which once more brings upon us the difficulty of explaining latent characters. Increase of nourishment, if it had any effect at all, would, we should think, tend to increase the number of gemmules, and thus to decrease the tendency to variation; but it has just the contrary effect. In the second case we must assume that the abnormal growth of the gemmules is due to the action of some external cause; and, therefore, that variation ought to show some sort of relation with changed circumstances; which can hardly be said to be the case. Still, this is conceiv-

able; our chief difficulty in receiving the hypothesis lies in believing in the existence of gemmules at all.

And yet there are reasons for thinking that variation is due to some physical and material cause; for, if it were not so, what are we to think of monstrosities—of animals born with two heads, or without any legs? and how else can we conceive of a necessary correlation between two different parts? But the strongest evidence that variation is due to some physical force lies in the fact that we are able to modify it to some extent; to increase or to decrease it by altering the amount of food. It is true that we can do this only to a very small extent; yet the fact of being able to influence it at all is a sufficient proof of its material nature. At present we can only accept inheritance and variation as observed facts, the explanation of which is beyond the pale of Darwinism.

In 1859 the mystery of mysteries was the origin of species. It was Darwin who showed us how simple is the explanation of that mystery; but he only pushed it one step back. The mystery of mysteries is still confronting us, as inscrutable as ever, and is now called "transmission of characters." But what a change the gaining of this single step has made! When shall we take another? We have just seen that the transmission of characters, and the origin of variations, in all probability depend upon physical causes; and, if this is the case, we may hope that they are not beyond the power of the human intellect to understand; and therefore we may hope that they will be explained. The last step would be the explanation of the nature and origin of life itself; but this appears to be quite unfathomable by our senses. No one as

yet has given us even a conceivable explanation, much less a possible one. It is a problem that will probably always remain shrouded in mystery, although no doubt the human intellect will always be trying to solve it.

And now, ladies and gentlemen, I have explained to the best of my ability the scope and limits of Darwinism. All through the lecture I have felt the difficulty of expounding clearly so vast a subject in so short a time; and I could have wished it had been placed in other hands. It was necessary for me to be concise, in order not to leave out any important point; and if at times I have been dry and obscure, I will ask you to remember this necessity for condensation, and to believe that I have tried to make my language as plain and as intelligible as possible.

LECTURE II

THE NEW DARWINISM

ELEVEN years ago I gave a lecture to the members of this Institute, in which I explained what was then understood as Darwinism; and I have now undertaken to try to explain the modern views held by Darwin's followers. Unfortunately it is a subject which, at the present time, is in a very confused condition, owing to the innumerable irrelevant issues that have been raised around it; but, when all the husk has been stripped off, the new Darwinism will be found to be a simple affair, easily understood.

This confusion is largely due to its history; for the new Darwinism did not come upon us in a flash, as did the old, but has been gradually assuming shape for many years.

In 1868 Moritz Wagner read a paper to the Royal Academy of Munich, in which he maintained that, without the separation of a few individuals from the rest of the species, natural selection could not act, and that these individuals must always have been isolated by geographical barriers. His thesis could not be upheld on either point, although it brought the importance of isolation prominently forward; and we may place the birth of the new Darwinism in November, 1886, when Dr Romanes' first paper on physiological selection was published by the Linnean

Society of London; for this contained a new idea. From that time it has gone on gaining coherence; and last year [1897] it received its latest development in the third part of "Darwin and after Darwin," also the work of Dr Romanes.

This new Darwinism has not destroyed, or in the least damaged, the old. Indeed, it is not antagonistic to it in any way. It is merely an addition; but one of great value; for, as I shall explain at the end of the lecture, it enlarges much our conception of the great scheme of nature. The doctrine of organic evolution, which was firmly established by Darwin, is now regarded as a fact by all scientific men; and his theory of natural selection, as a method of progressive evolution, is accepted, to a greater or less degree, by most naturalists. Darwin himself, as I reminded you in my former lecture, never thought that his theory was complete. He always taught that natural selection had been the main but not the exclusive means of modification. He knew that something was wanting; and it is that something which the new Darwinism attempts to supply.

THE NEO-DARWINIANS AND THE NEO-LAMARCKIANS

The Neo-Darwinians, as we are sometimes called, accept Darwin's teaching, and supplement the theory of natural selection with *methods of isolation*, which had been either overlooked or had not been brought into sufficient prominence by Mr Darwin. Those naturalists, who hold the opinion that natural selection has been of little importance in evolution, call themselves Neo-Lamarckians; for they think that the

teaching of Lamarck is nearer the truth than that of Darwin. And, in order that we may set out with clear notions of the differences between these two schools, it will be necessary for me to remind you, shortly, of the leading points in each; although I do not intend to go again over quite the same ground as I took you before.

The first great problem to be solved in organic evolution is the fact of progression upwards, from the simplest organisms to the most complicated. Progress implies a directive or determinative agency; and the question which presents itself is this:—Are the variations definite, that is, in a single direction, as they arise? Or are they indefinite, that is, in many directions; and has progress been secured by some subsequent selection of certain variations to the exclusion of others? In other words, can we see the elements of progress in the variations themselves? Or must we look for progress to some process which comes into operation after the variations have been formed?

Now Lamarck's theory is one of definite variation. He taught that progress was due to an internal tendency towards progression, combined with the action of external conditions on the organism, and with the more or less frequent use of the different organs of the body, due to different wants, which were caused by changed habits. For example, he supposed that the constant stretching of the toes of the otter in the act of swimming had caused the skin between the toes to enlarge; that constant running had made deers and antelopes fleet; and that constant stretching of the neck of the giraffe had made it grow.

The variations that arose in this way must therefore be definite or progressive, and must have been acquired after birth. He had only to assume that these acquired characters were transmitted to the next generation, and his theory was complete. The Neo-Lamarckians agree that progress has been mainly—although perhaps not exclusively—secured by the inheritance of post-natal variations, which have arisen, either through the attempts of animals and plants to adapt themselves to external conditions, or by the direct action of external causes on the individuals. The rest they refer to natural selection, the idea of an internal tendency towards progression being generally discarded.

Darwin taught that post-natal variations, due to the direct action of external conditions, are seldom, if ever, transmitted; and he held that progress has been chiefly due to the picking out of certain indefinite variations by the process of natural selection. Lamarck's theory, therefore, includes both the origin and the preservation of variations; for the variations are preserved by the conditions which called them into existence; while Darwin's theory relates to the preservation and development of variations only, and makes no attempt to explain their origin. Even with reference to the preservation of variations, the two theories are not exactly antagonistic; and they might possibly flourish side by side; for natural selection, although favourable to the hypothesis of indefinite variation, is not opposed to that of definite variation. It is only a process of picking out useful variations, no matter how they have arisen.

It may be thought that it would be easy to test

the truth of these two hypotheses by observation. If variations are indefinite, they will range themselves equally on each side of a centre; while of definite variations the majority would tend in one direction. But hitherto all attempts to settle this point have failed. Variation in domesticated animals and plants appears to be indefinite; but it does not necessarily follow that it is so in a state of nature. We have a good example of this in the dimensions of the leg-bones of the moas which were found at Kapua and Enfield.[1] Here the majority of cases undoubtedly show indefinite variation; but there are several exceptions; and I could not give a decided opinion on the subject; because, as Professor Lloyd Morgan has pointed out, variation apparently definite may be due to the action of selection.

Also it has been found impossible to prove conclusively that post-natally acquired characters can be transmitted. When we examine a series of fossils which show progressive development, we cannot feel sure that this development has been due to definite variation, unless it appears that the modification has not been of any advantage to its possessor; for, if the modification was advantageous, it might have been brought about by natural selection. And in those cases where the characters appear to be useless to their possessor, we cannot prove that they are of post-natal origin; for definite variations may be congenital, and due to some unknown factor in the laws of growth. The fundamental difference between Lamarckism and Darwinism lies in this—that for the

[1] See the diagrams in "Transactions," N.Z. Institute, vol. xxviii., pp. 643-650.

former it is necessary that post-natal variations should be capable of transmission to the next generation, while to the latter such transmission is not necessary. Unfortunately we can find no crucial case of such naturally acquired characters being transmitted; for all must be useful, and all must therefore be capable of being explained by natural selection; and there is no undisputed evidence that artificially acquired characters are ever transmitted. There is, therefore, no proof that post-natally acquired characters can be transmitted; and the evidence is strongly against such being commonly the case.

THE INSUFFICIENCY OF NATURAL SELECTION

We may now proceed to examine the difficulties in the way of considering natural selection as the sole agent in evolution; and we shall then be in a position to appreciate the amendments proposed by the new Darwinians.

The first difficulty is the existence of numerous characters, either of colour or of form, which are called non-utilitarian, because they are not of any use to their possessors. Darwin always saw this difficulty; but at first he thought that it could be overcome. In the first edition of the "Origin of Species" he attempts to explain these useless characters, as being principally due to the inheritance of characters, which were useful to ancestors of the present possessors, but partly as the results of correlation of growth. That is, he thought that they might be necessarily connected with other characters which are of use. No doubt the first explanation will account

for the presence of useless rudimentary organs or vestiges; but they are comparatively few; and still fewer are those characters which we may suppose to be due to correlation of growth. The great majority remain unexplained. Ten or twelve years later Darwin recognised the force of this objection; and in the "Descent of Man" he says:—"In the earlier editions of my 'Origin of Species' I perhaps attributed too much to the action of natural selection or survival of the fittest. I have altered the fifth edition of the 'Origin,' so as to confine my remarks to adaptive changes of structure; but I am convinced, from the light gained during the last few years, that very many structures which now appear to be useless will hereafter prove to be useful, and will therefore come within the range of natural selection. Nevertheless, I did not formerly consider sufficiently the existence of structures which, so far as we can at present judge, are neither beneficial nor injurious; and this I believe to be one of the greatest oversights as yet detected in my work. It is, as I can now see, probable that all organic beings, including man, possess peculiarities of structure which neither are now, nor were formerly, of any service to them, and which therefore are of no physiological importance."[1]

Dr Wallace, however, still argues that these characters are not really useless; but that we suppose them to be so, because we are so very ignorant of the habits of animals. He thinks it is impossible to prove that any character is not, nor ever has been, useful to its possessor, or that it is not necessarily correlated with some useful character; and he has

[1] "The Descent of Man," 2nd edition, p. 61.

himself pointed out that many of these so-called useless characters are useful as recognition-marks; that is, marks by which the individuals of a species are enabled to recognise each other. This theory of recognition-marks is probably true with reference to some of the higher animals; but it is very improbable that either form or colour could be used as recognition-marks among the lower animals; and the theory obviously does not apply to blind animals or to plants. Even among animals possessing eyes, there are many characters which cannot be regarded as recognition-marks; for they cannot be seen: as, for example, the teeth on the tongues of snails, and the internal convolutions of the suture in the shells of ammonites. The neuration of the wings of moths and caddis-flies is obscured by scales or hairs; and yet it often furnishes good generic and sometimes specific characters. Some crabs are always covered with sea-weeds growing on their shells; and the species to which they belong cannot be ascertained until these sea-weeds have been removed. And generally, obscure characters cannot be explained as recognition-marks, when there are conspicuous characters to answer that purpose. These specific characters, therefore, are not recognition-marks; are they adaptations of any other kind? We can hardly suppose that the colours which distinguish the shells of some bivalves which live buried in sand have any adaptive value. Nor can we suppose that a spine more or less, or a different arrangement of the tubercles on the carapace of a crab, can give one individual an advantage over another. Neither can it matter in the struggle for life whether the nervure in the wing of an insect

branches once or twice. Again, can we suppose that slight differences in the number or shape of the teeth in snails have any adaptive value? Or take the shape of the spicules in sponges, or the small differences in the leaves of ferns and mosses, or the various patterns of ornamentation on the valves of diatoms: can all or any of these characters be explained by the law of utility? No one, I believe, is at present prepared to maintain that they can.

But it will be as well to give a few special illustrations. The Radiolarians are minute, microscopic animals, whose soft body is supported by an internal siliceous cage, or skeleton, of great variety of form. Now these different forms cannot be correlated with each other, for each species has its own; and they cannot be recognition-marks. The skeleton, of course, is useful to the animal as a support, and as a defence against enemies. But several of the forms which we find in living Radiolarians we also find in Radiolarians which lived before the commencement of the Palæozoic era. During almost the whole history of life on the earth these different forms of skeleton have been competing with each other. During this competition many other forms of skeleton came into existence; but they did not vanquish the old ones; all lived and multiplied alongside each other, without any one obtaining the victory. Obviously, the particular form of the skeleton cannot give any advantage, and cannot therefore be due to natural selection.

The Radiolarians are amongst the lowest of organisms. Now let us go to the highest class for an illustration. The different species of elephants, both living and extinct, are distinguished, among other

characters, by the different ways in which the enamel and dentine of the molar teeth are folded into transverse ridges. This folding of the hard enamel, and the filling of the interstices with a softer cement, is a very useful structure; for, owing to the different hardness of the different parts, an uneven surface is constantly maintained, which enables the tooth to fulfil its function of grinding down leaves and shoots of trees, on which both the species of living elephants feed. In the African elephant the transverse ridges are thicker in the middle than at the ends; while in the Indian elephant they are equally thick all through, and the number of ridges in each tooth is nearly double the number in those of the African elephant. In the Mammoth this type of tooth was still more specialised than in the Indian elephant. Now extinct elephants of both types lived together in India during the Pliocene period. In the Pleistocene the African elephant lived in South Europe with other species, some of which belonged to the Indian type; and at the close of the Pleistocene, the Mammoth, which had the most specialised teeth of any, became extinct. As these two forms were competing for so long a time, without either of them gaining any advantage, we cannot suppose that the two living species of elephant owe their preservation to the superior pattern of their molar teeth. The fact is that one pattern is as useful as the other, neither more nor less; and, as no advantage is gained by having different patterns, they cannot have been developed by natural selection.

I am tempted to give one more illustration, taken from the class of birds.

There is a genus of small fruit-pigeons, called *Ptilonopus*, found in the Malay Archipelago and Polynesia. It contains twenty-three species, of which no fewer than thirteen are found, isolated from other species of the genus, each in its own island or small group of islands; the other ten living associated, two or more species together. Now it is highly improbable that the whole of the thirteen isolated species were developed on other islands from which they have migrated, and that their ancestors, who were left behind, have all become extinct in their former homes. Also, it is highly improbable that formerly two or more species of *Ptilonopus* existed on each of these thirteen islands, and that all have been destroyed except one species in each island. Either one or other of these cases might happen occasionally; but it is impossible that thirteen should occur simultaneously. Consequently, it appears certain that most of these species were developed singly, each on its own island. If this be the case, the colours which now distinguish the different species cannot be recognition-marks, because there is no other species in each island with which they could be confounded. The colours cannot be due to correlation, because they are the only characters which have changed. They cannot have been useful to ancestors, because they have only lately been developed. And we cannot suppose that they give any special advantage in each island, because all the islands have practically the same climate and the same flora and fauna. This exhausts the resources of the principle of utility; and we are driven to the conclusion that these specific characters have a non-utilitarian origin. And, if the colours have not had a utilitarian origin in

these isolated species, it is quite probable that they may not have had a utilitarian origin in other cases, where two or more species of the genus are found together. Therefore it follows that recognition-marks and other specific characters do not necessarily arise through natural selection. To form these non-utilitarian characters something else must have been at work.

A second objection is sometimes made, that incipient variations, even if they are useful, cannot be of any importance in the struggle for existence; and that natural selection could not develop them until they had made a considerable advance. This is quite true; but it is only another form of the first difficulty; for, until the variations have attained to what is called "Selection Value," they are merely useless characters.

The third difficulty is that natural selection, although a powerful cause of divergence between different species and genera, cannot by itself initiate divergent evolution. This will come as a surprise to many of you, who have learnt from the writings of Dr Wallace that natural selection is the one great cause of divergence. Nevertheless it is true that natural selection cannot make one species divide into two, but actually prevents such a thing from coming about. Natural selection hinders variation, except in that direction which is the one most profitable to the whole of the individuals; and, when the external conditions remain the same for a long time, it entirely destroys all new variations.

Darwin, when discussing the question of divergent evolution, says: "As has always been my practice, let us seek light on this head from our domestic productions." After stating that fanciers of pigeons

choose different characters, he asks, "Can any analogous principle apply in nature?" and he answers, "I believe it can and does apply most efficiently, from the simple circumstance that the more diversified the descendants from any one species become in structure, constitution, and habits, by so much will they be the better enabled to seize on many diversified places in the polity of nature, and thus increase in numbers." [1]

But Mr Darwin here forgets that the pigeon-fancier not only selects his birds, but he isolates them from each other; while natural selection does not isolate different individuals of the same group. In another place he truly says, "intercrossing plays a very important part in nature, by keeping individuals of the same species, or of the same variety, true and uniform in character;" and he evidently overlooked the fact that this will prevent a species branching off and occupying diversified places in the polity of nature, unless there is some means by which the different varieties become isolated from each other, and thus escape from the effects of inter-crossing.

Mr Darwin tacitly acknowledges the failure of natural selection to produce divergence in the case of insects inhabiting islands. After pointing out the dangers encountered by flying insects inhabiting small islands, he says: "When a new insect first arrived on the island, the tendency of natural selection to enlarge or to reduce the wings would depend on whether a greater number of individuals were saved by successfully battling with the winds, or by giving up the attempt and rarely or never flying." [2] Here he

[1] "Origin of Species," 1st edition, p. 112.
[2] "Origin of Species," 1st edition, p. 136.

recognises that there would be no splitting-up into two varieties, but that only the commoner one would be preserved, although both were useful. Nevertheless, this very case is given by Dr Wallace as an example of divergence under the action of natural selection.[1]

Some years later Mr Darwin saw this difficulty more clearly; for in a letter to Moritz Wagner, dated October 13th, 1876, he says: "I do not believe that one species will give birth to two or more new species, so long as they are mingled together in the same district."[2] And on November 26th, 1878, he thus wrote to Professor K. Semper: "There are two different classes of cases, as it appears to me, viz., those in which a species becomes slowly modified in the same country, and those cases in which a species splits into two, or three, or more new species; and in the latter case I should think nearly perfect separation would greatly aid their 'Specification,' to coin a new word."[3]

Dr Wallace, however, still thinks that natural selection can produce divergence; but all the illustrations he gives in his book on Darwinism either include isolation, or they start with two or more different species, and so miss the point, which is that one species cannot give rise to two without the help of isolation. Also, when discussing the influence of natural selection on infertility, he says that, "if two forms of a species freely inter-cross with each other, and produce mongrel offspring which are quite fertile

[1] "Darwinism," 2nd edition, p. 105.
[2] "Life and Letters," vol. iii., p. 159.
[3] "Life and Letters," vol. iii., p. 160.

inter se, then the further differentiation of the forms into two distinct species will be retarded or perhaps entirely prevented."[1] So, to help him in his argument, he assumes that the mongrel offspring are not quite fertile *inter se*.

But if the two forms are thus liable to obliteration, how could they possibly have arisen? The partial sterility must have commenced before the two forms could have been produced, and therefore before there were any mongrels. In other words, certain individuals must have been more or less physiologically isolated from each other before the two forms could have come into existence. This supposed example of Dr Wallace does not, therefore, explain the difficulty, which is the origination of two forms from one, without the help of isolation.

As this is a very important point, it will be better to illustrate it further. Let us take the simple case of the first organisms, floating on the surface of the sea, and belonging to one species only. We must, however, suppose—which was not really the case—that they increased in numbers by conjugation and not by self-division; for, unless we make this supposition, we cannot reproduce the action of natural selection without the aid of isolation. There are reasons for supposing that the primeval ocean contained mineral hydro-carbons, which would slowly oxidise into carbohydrates, and thus furnish food for these first organisms.[2] But this supply of food was limited; and in time it would be necessary for some individuals to

[1] "Darwinism," 2nd edition, p. 174.

[2] "Report of the Australasian Association for Advanced Science," vol. vii., Sydney (1898). Presidential Address to Section C.

develop chlorophyll, so that they might obtain their supply of carbon from carbonic acid when the carbohydrates were consumed. Other individuals, however, would lead a more animal life, and live upon their neighbours. But if free inter-crossing was maintained, these two types could not completely differentiate; for they would always be getting mixed up again; and the whole group of organisms would progress in that single direction which was, on the whole, the most advantageous for them. Evidently they could not become entirely animal, for they would then lose the power of obtaining food from the mineral substances around them; and they would not become entirely vegetable, because the power of digesting organic substances, either living or dead, would certainly be advantageous; as also would be the power of moving about in quest of this food. So that, on the supposition that free inter-crossing took place, the original organisms could not have separated into plants and animals. Just as a river cannot branch on its way down its valley, until the sea throws up a mud-bank at its mouth and forms a delta; so natural selection can only propel a species in one direction, unless isolation steps in and divides it into smaller groups.

A fourth objection has been urged, that natural selection cannot explain the mutual sterility between individuals of different species. So far as our present experience goes, it seems that complete fertility between individuals of different species is rare, and that nearly all are absolutely infertile. No matter what the characters may be which distinguish the species—whether it be form or ornament, or mere colour—this infertility is almost always present; and of all the

innumerable differences which separate species, it alone is constant, or nearly so.

In my former lecture I pointed out that Darwin had shown that sterility between species was not absolute, as had previously been supposed; and therefore it was not a fatal objection to the theory of the development of species. But, although he did this, he failed to give any satisfactory explanation of the facts; and he himself did not believe that infertility could have been caused directly by natural selection. He suggested that the changes in structure were necessarily followed by physiological changes; and, as neither the great structural changes which have taken place in domesticated animals, nor their exposure to greatly changed conditions of life—owing to man having taken them to different parts of the world—as neither of these things have produced infertility, Darwin had to add that, to bring it about, a very long time was required, together with exposure to uniform conditions. But these suggestions, which rest on no evidence, are quite insufficient to account for so important and so wide-spread a phenomenon; and they imply that all incipient species or varieties are quite fertile among themselves; a conclusion which Darwin himself says is not correct.

In the later editions of the "Origin of Species" he is still more explicit. He says: "After mature reflection it seems to me that this (sterility) could not have been effected through natural selection;" and, after giving some illustrations, he adds: "But it would be superfluous to discuss this question in detail; for with plants we have conclusive evidence that the sterility of crossed species must be due to some principle, quite

independent of natural selection . . . and from the laws governing the various grades of sterility being so uniform throughout the animal and vegetable kingdoms, we may infer that the cause, whatever it may be, is the same or nearly the same in all cases." [1]

Now our proper answer to this objection is, that it has nothing to do with Darwinism, which is a theory of the origin of species and higher groups by the preservation of variations; and does not attempt to explain their origin, whether they be physiological or morphological. The reason that the subject has been included in Darwinism is that Darwin so included it himself; for he at first thought that it could be explained by natural selection; and all other authors have followed him; indeed, Dr Wallace still maintains that natural selection can produce sterility.[2] His argument, however, is founded on the old but erroneous idea that natural selection implies the exclusive breeding of like with like; that it is strictly the survival of the best fitted, and not the destruction of the least fit. Also he has to assume that the morphological and physiological variations arise simultaneously, and that the former is of sufficient importance to be of selective value—two very improbable assumptions. It is indeed certain that natural selection could not have produced or even favoured sterility; for it could never be profitable in the struggle for existence to decrease fertility, unless that decrease was more than compensated for by extra protection given to the young.

This objection may therefore be dismissed, as lying

[1] "Origin of Species," 6th edition, p. 248. See also "Life and Letters," vol. iii., p. 80.

[2] "Darwinism," 2nd edition, p. 174.

outside our subject; for the Darwinian theory need not explain the mutual sterility between different species. From our point of view infertility can only be looked upon as a means of isolation; and, as such, I shall refer to it again presently.

ADDITIONS TO THE OLDER THEORY

Let us see how the difficulties which I have mentioned have been met by the new Darwinians.

Darwin says that his idea of natural selection was originally founded on the analogy between species in a state of nature and the races of plants and animals domesticated by man. He examined how these races had been brought into existence, and found in the principle of Malthus—the principle that population always tends to overtake the food supply—a process acting in nature, which was, he thought, analogous to human agency; and it was this that he called natural selection. Now the artificial selection by man includes, as I have already said, two different processes:—first, the individuals, and, secondly, their separation or isolation from all others with which they might inter-breed. These two processes are quite distinct. We can select one or more individuals without isolating them; or we can isolate one or more individuals without selecting them. It is the isolation which preserves the variation, selection being directive only. But in natural selection no such division can be made; so that it does not cover the same ground as artificial selection.

Of course, Darwin was quite aware of this; indeed he expressly mentions it. "The prevention of free crossing," he says, "and the intentional matching of

individual animals, are the corner stones of the breeder's art." [1]

But his attention was so much occupied with establishing the theory of natural selection, in which the act of selection also isolates, that he overlooked the fact that, without some other form of isolation, his analogy was incomplete, and that natural selection alone could not attain to any great importance. Sometimes he half recognises this, as when he says [2]: "Isolation is an important element in the process of natural selection," for "intercrosses with the individuals of the same species will be prevented" by its means. But he also adds that "isolation probably acts more efficiently in checking the immigration of better adapted organisms; and thus new places in the natural economy of a country are left open for the old inhabitants to struggle for, and become adapted to, through modifications in their structure and constitution." Again he says, "Although I do not doubt that isolation is of considerable importance in the production of new species, on the whole I am inclined to believe that largeness of area is of more importance."

These quotations show that Mr Darwin had not attained clear ideas on the subject. But with him isolation was limited to geographical isolation; and from this point of view his remarks may be correct. He failed however to see that isolation of some kind or other, on the larger areas of land, was necessary to allow natural selection full play. It was the recognition of this by Moritz Wagner, Dr Romanes, and others,

[1] "Animals and Plants under Domestication," 2nd edition, vol. ii., p. 62.

[2] "Origin of Species," 1st edition, p. 104.

that gave rise to the new Darwinism; which, put into a nutshell, is the raising of isolation to its proper place in the Darwinian theory.

This isolation, which is so necessary to prevent the effects of free inter-crossing, has been found on investigation to be a very complicated affair. In the first place, it is important to notice that the individuals which are isolated from the rest may be taken haphazard, so far as any external characters are concerned; or the possession of some special external character may be the cause of their being isolated. The first of these two cases is called *indiscriminate isolation*, that is isolation without selection; while the second is called *discriminate isolation*, because the isolated individuals are discriminated or selected from others by the possession of some special morphological character. Discriminate isolation leads to progress in the direction of the characters which originally caused the isolation. The effect of indiscriminate isolation must depend very much on two things: (1) the number of individuals isolated, and (2) their inherent capacity for variation, which is known to be very different in different species. The more variable the individuals are, the greater is the chance of variations arising among them; and the fewer they are, the greater is the chance of a variation being preserved.

In the second place, isolation may appear under various forms; and this leads to the necessity for classifying these different forms into groups. In treating this part of my subject I must beg your forbearance; for, although classifications are indispensable if we wish to attain clear ideas, they are apt to be a trifle dry; and I am afraid that I may weary you while

enumerating them. I could give you a general account without entering into details; but you would be left with hazy notions on the subject; and, if you will forgive me when it is over, I will get through it as quickly as I can.

I will take first those cases in which isolation is alone concerned, and which therefore are indiscriminate. We can divide them into physical isolation and physiological isolation, according to the nature of the barrier which isolates the groups of individuals. I will then take the cases in which isolation is accompanied by selection, and where, in consequence, the isolation is discriminate. Under this head we have natural selection, in which the selection is involuntary and cannot be obviated; and preferential selection, in which selection is a voluntary act of the individuals and not absolutely necessary. These two kinds of selection may be called *intrinsic*, as the individuals select themselves or form themselves into groups for their own pleasure or profit. But there is another kind of selection—analogous to the artificial selection by man—where the individuals are selected by an outside selector, who forms the groups for *his* benefit or amusement, and not for the benefit of the selected. This I will call *extrinsic* selection.

It may be, and indeed often is, the case, that two or more of these processes may combine. Either physical or physiological isolation may preserve variations which are afterwards expanded by natural selection; or natural selection may modify one part of a plant while extrinsic selection is modifying another.

To commence then with PHYSICAL ISOLATION. This was known, under the name of geographical isolation,

before the publication of the "Origin of Species;" but the principle has since been explored in all directions by Moritz Wagner, Professor Weismann, and the Rev. J. Gulick. It arises when a group of individuals is prevented from mixing with the rest of the species by some physical barrier which they are either unable or unwilling to cross; and is chiefly due to migration, either voluntary in search of food, or involuntary, as when plants or animals are driven by winds or currents from one place to another. Its completeness depends largely upon the power of locomotion possessed by the individuals; for two snails would be effectually separated by a distance which would appear as nothing to a bird. The simplest form of physical isolation is when a few individuals cross the sea from the mainland to an island, or from one island to another; and in time, owing to the appearance of some new variation, gradually develop into a new species. A few examples will be of interest.

The oranges of Tahiti have developed into a very distinct variety; the fruit being smooth, yellow, and round, and with a different flavour from any of the oranges of Australia or Europe. Professor H. C. Bumpus has given us very interesting evidence of varietal changes which have taken place with great rapidity in the United States. In a lecture delivered at the Marine Biological Laboratory of Wood's Holl, Massachusetts, in 1896, he says that between the years 1850 and 1870 more than fifteen hundred European sparrows were introduced into the United States, and have since spread over the continent. On comparing a large number of the eggs of these introduced sparrows with others from Britain, he finds

that the American eggs are more variable both in shape and colour, and that they have become smaller and rounder than British eggs. Unfortunately, he could not compare the birds, on account of the expense of procuring a sufficient number of skins. He has also compared 3000 specimens of the shells of the British Periwinkle (*Littorina littorea*) with 10,000 North American specimens of the same species, which was only introduced into America about fifty years ago; and he finds that the American shells are more elongated, lighter in weight, more bulky, and the colour-markings less pronounced than in British specimens.[1] Professor Bumpus thinks that these variations are due to the animals having been subjected to a new environment, and thus emancipated from many of the old restraining influences of natural selection. But, however this may be, it is certain that these changes would not have taken place if the animals had remained in Britain; and, whether the variations have or have not been produced by external agencies, we may fairly say that they are due either directly or indirectly to physical isolation; for in either case isolation was necessary for their preservation.

It is evident that, if these changes continue, new species will arise. This appears to have been the case with the adult insect of the slug-worm which infests our pear and cherry trees; for it has been described as a new species (*Monostegia antipoda*) by Mr W. F. Kirby, although, no doubt, it was introduced from the northern hemisphere. There is also ample

[1] "Zoological Bulletin of the Marine Biological Laboratory of Wood's Holl," vol. i., No. 5.

evidence that specific changes have occurred over and over again in the past. We need not go beyond New Zealand for examples.

The Crow (*Glaucopis*) of the North Island has its wattles coloured blue, while those of the South Island are half blue and half orange. Our Tits (*Myiomoira*) have a yellow breast, and those in the North Island a white one. The Thrushes (*Turnagra*) and Robins (*Miro*) also differ in the two islands. We cannot suppose that these different species formerly inhabited different districts, when the two islands were united, that is before Cook's Strait was formed; because part of the South Island lies to the north of part of the North Island; and, if the two islands were again united, the birds of Nelson and Wellington would certainly mix together. Some of these species must evidently have been formed since the islands were separated, and cannot be due to new external conditions. If we turn our attention to the Chatham Islands, we find that the Bell-bird (*Anthornis*), the Fern-bird (*Sphenœacus*), the Warbler (*Pseudogerygone*), and the Pigeon (*Hemiphaga*), all differ from their relations in New Zealand. I have not time to discuss all these cases, but will take the Chatham Island Pigeon as an example. It differs from that of New Zealand in having the lower part of the back and the outer wing-coverts gray instead of purple. We have no reason to suppose that these colours are in any way related to the environment. We have no reason to suppose that the Chatham Island bird could not live equally well in New Zealand, or that the New Zealand pigeon would be under any disadvantage in the Chatham Islands. Consequently, we have no reason for

thinking that these differences of plumage are of any special use to the birds, or that they could have been produced by natural selection; and the only explanation appears to be that physical isolation has preserved fortuitous variations which arose subsequently to the isolation.

One of the best examples of the effects of physical isolation is found among the Albatrosses. There are twelve different kinds of Albatrosses which roam over the Southern Ocean, mixing freely together; and nine or ten of these frequent the Tasman Sea; but each has its own separate breeding place, to which it retires every year. Now as these birds have no enemy but man, and as they all have the same habits, and live on the same food, their specific characters cannot be connected with the struggle for existence. Neither can they be due to external conditions; for all live together except at the breeding season. We cannot, therefore, suppose that each species was formed by competition on the ocean, and that each subsequently chose a separate breeding ground. Or, in other words, we cannot suppose that the development of their specific characters preceded their isolation. Evidently isolation preceded and was the cause of the preservation of variations which subsequently arose, and in time became of specific importance. It is possible that the differences between the species may now be useful as recognition-marks; but if they arose subsequently to isolation, the birds must at first have got on very well without them; and, if so, there is no reason for supposing that recognition-marks are wanted now.

Of course, a very large number of cases could be

cited where plants and animals have been long isolated without undergoing any known change. The house-fly and honey-bee, for example, remain unchanged in all countries. But this only shows that many plants and animals do not change readily, and that there have been no variations for isolation to preserve. For, once more, I must remind you that isolation does not induce variations; it only preserves them if they arise. It is well known that some plants and animals vary more than others; and it is usually the common species that vary most. I am of opinion that variable species form but a small minority of the whole, and that non-variable or slightly variable species form the immense majority. If it were not so we should not be able to define species so easily as we can; for most species are easily defined, far more easily than genera; a truth which was more widely recognised forty years ago than now.

Another form of physical isolation is due to migration in search of food. This is often accompanied by a change in habits, which keeps a small group of individuals together. For example, in the southern parts of New Zealand the introduced trout (*Salmo fario*) has taken to live in the brackish water at the mouths of rivers, or even in the sea, and has developed into a new variety, unlike any in England from whence their ancestors came. Sometimes a distinctly useful variation may arise; and then it would be increased by natural selection. And it is, no doubt, to the co-operation of natural selection with physical isolation that we owe the peculiar colours of animals living in deserts and, indeed, all cases of protective coloration may be explained in the same way.

The change from diurnal to nocturnal habits may

be considered as another case of physical isolation; although the two groups are separated only as Box was from Cox—one group sleeps while the other feeds. Nocturnal habits have been assumed as a protection against enemies by many very different kinds of animals; but in other cases the change has been made in order to secure food. The slow-moving Lemurs, for example, could not catch birds in the day time; but at night they steal silently along the branches and seize them when asleep. The Owls, as a group, must in the first instance have become isolated by adopting nocturnal habits; but their division into a number of genera and species has come later, and is due to other causes. Some of the characters which divide them into subordinate groups are distinctly adaptations—such as the serrated claw on the middle toe, the operculum of the ear, the longer or shorter wings, &c.—but a large number of the lesser characters can have no utilitarian value.

Physiological Isolation is when certain individuals of a species are prevented from inter-crossing by physiological causes, although they freely mix together during the breeding season. The simplest form of it is found in the lowest plants and animals which have no sex, but increase their numbers by self-division. This insures that each individual is isolated from all others; and consequently any variation that may arise will be preserved, unless it is harmful to the individual, or is counteracted by reversion. To this, no doubt, we owe the original separation of plants from animals; as the variations which gave rise to this separation were useful, they would be taken in hand by natural selection, and so would become progressive. We may also find

in asexual reproduction the cause of the immense variety seen among the Diatoms, Fungi, Radiolarians, and Foraminifera. These variations, some of which are the very earliest recorded in the rocks, were non-utilitarian, and never progressed far, probably because further progress in any direction would have been harmful.

Self-fertilisation is nearly as efficient a cause; but a cross may occasionally occur. Ferns, and many other plants, as well as some animals, are thus isolated and able to preserve indifferent variations. A few years ago Count Berg Sagnitz made a large number of observations, with the expressed purpose of testing the truth of this deduction; and he found that self-fertilised plants really have more constant varieties than species which are cross-fertilised.

Another kind of physiological isolation is caused by a change taking place in the season of flowering, or in the time of pairing, whereby late and early individuals are prevented from crossing. There is a good instance of this in the Mutton-birds of the Kermadec Islands. There two varieties of *Œstrelata neglecta*—the Mutton-bird and the Winter Mutton-bird of the settlers—breed on the same island, but at different times of the year. The first has the neck and breast, and sometimes the whole under-surface, grey; while the Winter Mutton-bird has only a grey band on the breast, the rest of the under-surface being white. Here we see physiological isolation bringing about much the same result as physical isolation has done in the case of the Albatrosses; and we must suppose that the change in the time of pairing preceded the change of plumage.

It also appears that, even among individuals which

habitually inter-cross, physiological isolation may arise by the infertility of certain individuals with others. We know, as a matter of fact, that almost all species, even those closely allied to each other, are physiologically isolated by the ban of sterility. We have an excellent example of this in our Fan-tail Flycatchers (*Rhipidura*). The pied and the black Fan-tails are two allied species, of which the former is found throughout New Zealand, while the latter is confined to the south. In the neighbourhood of Christchurch we often used to see, when the birds were common, individuals of the two species courting each other; and Mr T. H. Potts has recorded several cases of what he called union-nests; but no one has ever seen a hybrid. Evidently the two species are mutually sterile. They are physiologically isolated from each other. Indeed, in nature we constantly find closely allied species, or even varieties of the same species, living together in intimate association; and yet hybrids and mongrels are most rare, especially among animals. Even in cases where hybrids or mongrels can, as we know, be produced under artificial conditions, they are not so produced under natural conditions.

The prepotency of some pollen-grains over others is another well-ascertained fact. It is known that if the pollen-grains from a number of different plants be placed on the stigma of a flower, the stigma will, in most cases, select, with unerring accuracy, the pollen of its own species or variety, and reject the others. If it were not for this, hybrids would be far more abundant in the vegetable kingdom than they are. But there are exceptions to the rule. In some plants the ovules absolutely refuse to be fertilised with pollen

from the same plant; but can be fertilised by pollen from any other individual of the same species. There are also very many species which are partially sterile with their own pollen; and there is a large number in which pollen from another individual or variety is prepotent over the plant's own pollen.[1]

Again, we know as a fact that, among domesticated animals, different individuals show variations in fertility and in the power of transmitting their characters to their offspring. Physiological variations in the reproductive cells evidently exist; but, as we cannot see them, and can only recognise them by experimenting, they form a very difficult study, and we know very little about them. What we do know leads us to suppose that physiological variations arise, and are transmitted in a similar manner to morphological variations. There is, however, this important difference: varieties which are mutually infertile cannot be swamped by inter-crossing; they preserve themselves.

The rarity of hybrids in nature is a most remarkable fact; for it shows us that this character of sterility is widely spread; and the question arises, How did it originate? Dr Romanes proposed an explanatory hypothesis, which he called "physiological selection," but which is more appropriately named "progressive infertility." He supposes that certain individuals became to some extent infertile with others, although they retain complete fertility among themselves; and that those individuals which are most fertile among themselves tend to become more and more isolated from other groups. It is thought that, if a slight amount of mutual infertility arose between any two individuals

[1] "Cross and Self-fertilisation in Plants," 2nd edition, p. 2.

of a group, among which free inter-crossing prevailed, the progeny of the less fertile pair, being fewer than those of the more fertile pairs, would be gradually eliminated in the struggle for existence, while the descendants of the more fertile pairs would become separate groups, not perfectly fertile with each other —all crosses between the two being eliminated. If now the same thing happened again among the members of one of the groups, another step would be taken; until at last we can conceive it possible that two or more groups might become completely isolated from the others by means of physiological variation and the struggle for existence. The small families would be crowded out by the members of the more prolific families, not because they were less adapted to their surroundings, but simply by force of numbers. It will be noticed that utilitarian characters are not selected, and no definite morphological modification is given to the survivors. Consequently there is no natural selection; and it is only because the infertile families have fewer offspring that they disappear altogether.

Dr Wallace has, however, shown that the physiological variety could rarely maintain itself by this process; and it is more probable that variations in infertility easily become definite; in which case mutual sterility might often arise, and thus individuals would become physiologically separated from each other. It seems possible that in this way groups have become isolated, although living in the same area; and natural selection could then act on any group without interfering with the others; and thus divergent evolution would follow. That sterility does, by some means or other, arise between species, is simply a matter of fact;

and the essence of Dr Romanes' hypothesis is, that he supposes that the physiological change precedes the morphological, instead of following it, as Darwin had thought. Let us now see what evidence can be produced in its favour.

In the first place, it is a fact that the different varieties of a species generally breed true, even when closely associated together; and from this we infer that the physiological changes which have taken place in their constitution are greater than the morphological changes. In the second place, sterility between species is the common character of almost all. It is found associated with all kinds of structural and colour characters; and yet, in the exceptions to the rule, we see that considerable structural and colour differences may arise without affecting the fertility of the individuals in the least. This seems to imply that physiological isolation has almost always come first, and that morphological characters follow; that infertility is the common originator of most of the species, and that structural alterations in different directions came later.

This inference is strengthened by the fact that the hypothesis also offers an explanation of the exceptions to the rule; for those species which are not mutually sterile may have been isolated by other means than that of progressive infertility; and, therefore, there is no reason why they should be infertile, unless the morphological changes which have since taken place are too great. The same explanation applies to our domesticated races; for they have not been selected for infertility, but for form, or for some other useful character.

The weak point of this argument is, that the explanation seems to go too far; because we can hardly

suppose that all sterile species have been isolated by progressive infertility. Darwin says, "Separate regions are often inhabited by groups of species, which, when brought together and crossed, are found to be more or less sterile, and consequently this could not have been effected through natural selection," [1] or, as we should now say, through progressive infertility. This reasoning, however, is not conclusive; for the ancestors of these separated groups must at one time have lived together, and the process of progressive infertility may have then come into operation. Still it seems probable that sterility eventually arises in most cases of physical isolation; but observations and experiments on the subject are much wanted.

That variations in infertility occur in nature is an undoubted fact; and that complete infertility sometimes precedes structural change seems to be the case with domesticated animals; for Darwin says, "It is by no means rare to find certain males and females which will not breed together, although they are known to be perfectly fertile with other males and females." [2] But isolation by progressive infertility will not be proved until it has been shown by experiment that, among animals living in a natural state, infertility exists between some individuals of the same species, which still retain complete fertility with others. In other words, it is necessary to prove by facts the supposition that infertility sometimes precedes morphological change.

[1] "Animals and Plants under Domestication," 2nd ed., vol. ii., p. 169.

[2] "Animals and Plants under Domestication," 2nd ed., vol. ii., p. 146.

Of course the new Darwinism does not profess to explain the origin of physiological variations any more than the origin of morphological variations. What Dr Romanes has done is to call attention to these variations, and to point out that one of them—variations in fertility—must be a very important factor in evolution, as it would produce isolated groups in those large areas where many individuals of a species are mixed together, and where they exist in large numbers.

Both physical and physiological isolation are indiscriminate in their action, and isolate non-utilitarian as well as utilitarian variations. If the variations are non-utilitarian, they do not go far. But if they should be useful or attractive variations, they will be seized upon and developed by one of the processes of selection which I must now describe.

We will first take NATURAL SELECTION, which isolates beneficial variations by killing off the others. It is not so much natural selection as natural elimination; for it works not by the survival of the fittest but by the destruction of the least fit. It is also a method for the evolution of adaptive characters only; because the elimination is for the benefit of those who remain, that is for the benefit of the selected; and its most appropriate name would perhaps be "utilitarian selection." It may be a struggle for food, or it may be a struggle for protection from enemies, or it may be a struggle to secure the persistence of the species; but in all cases it must be a struggle, with death as the penalty for being vanquished. Because, without elimination by death, there can be no selection and no isolation: the two here go together.

Natural selection also includes the greater part of Mr Darwin's "sexual selection;" everything in it which relates to the combats between males, and to the possession of particular structures for capturing the females, as well as to the organs or colours by means of which an individual of one sex makes known its position to those of the other sex. In these cases the unsuccessful candidates are not necessarily killed, but they are prevented from breeding; which, from our present point of view, amounts to the same thing.

It is, however, only the struggle for food which is brought about by the rapid increase of the members of a species. The struggle for protection and the struggle for perpetuating the species do not at all depend upon the doctrine of Malthus. On the contrary, the more individuals there are of a species, the less the necessity for securing special means of protection, and the less is the importance of the struggle for existence. But, in all cases, the power of natural selection, as a process of evolution, increases as the structures which influence the struggle get more perfect, and as competition gets keener. It can hardly come into play in the early stages of a variation, or where competition is checked by physical isolation; but it has increased in importance with the age of the earth, and is now the dominant factor in the evolution of genera and the larger groups among animals and plants.

Preferential Selection includes the selective association of Dr Wallace, and part of the sexual selection of Mr Darwin. By its means, groups are formed by individual preferences, due to the mutual attraction or aversion felt by some individuals for others of the same species. Of course we can only

expect to find its action among the higher animals ; and in biology it chiefly occurs in the highly nervous and æsthetic birds ; it is however very influential in human affairs.

Preferential selection differs from natural selection in that it is a voluntary act, and that the process is one of exclusive mating of the best varieties, and not the destruction of the worst. Consequently it can give rise to divergent evolution without the help of any other form of isolation. But it resembles natural selection in being discriminate ; because the isolation is produced by the attractive characters themselves, which consequently tend to accumulate. Some of the beautiful colour patterns of birds are probably its greatest triumphs ; but in the colours of squirrels, antelopes, and monkeys we can trace its effects among the more prosaic mammals.

Many facts in favour of selective association will be found in the second volume of Darwin's " Variations of Animals and Plants under Domestication," and in his " Descent of Man," as also in Dr Wallace's " Darwinism ; " to which I can only add that all cases of truly gregarious animals, that is of animals which live in flocks all through the year, should be included under this head. I will, however, give you an illustration of how important a tidy dress may be to a bird. Mr R. Henry, the Government Conservator of Native Birds in the West-coast Sounds, mentions a male Maori-hen (*Ocydromus brachypterus*) which was weak and in bad plumage when it first came about his hut, and was always thrashed and hunted away by one of the female Maori-hens. He fed the poor bird up ; and when it had acquired a brand new suit of feathers, it

actually captivated and paired with the very hen that used to hunt it about. Indeed, Mr Henry says that neither males nor females have a chance of getting mates while they are disfigured by having their feathers pulled out when fighting.[1]

There is still another form of isolation to be considered, which I have called EXTRINSIC SELECTION; for it implies the presence of an agent as selector, who is outside the group of individuals from whom the selection is to be made. This process differs from natural selection in that the rejected individuals do not perish, but are left alone; while the selection is made for the advantage of the selector, and not for that of the selected individuals. Mr Darwin included it under natural selection; but its mode of action is so different that it is entitled to separate recognition; for the process is analogous to that used by man in forming the various races of domesticated animals and plants. It includes only a few instances among plants and animals in a natural state; and in every case the selector is an insect. For this reason the Rev. G. Henslow has called it "insect selection;" but a term which will also include selection by man seems preferable, especially as it emphasizes the difference between these and other methods of selection; and some such term is indispensable when discussing the action of selection in human affairs.

Under this head then we have, first, certain insects kept by ants. The nests of ants are frequented by many insects, partly on account of the warmth, partly for the food they find in the nests. The ants also in some cases gain advantage from their visitors, who

[1] *Trans.* N.Z. Institute, vol. xxx., p. 279-280

supply a liquid food which they secrete; and the ants sometimes look after the progeny of these visitors as carefully as if they were their own. Indeed, several kinds of beetles have not only been domesticated, but have been subjected to selection until some have become blind, and others are unable to feed themselves. These variations are evidently injurious to the domesticated animals; but are useful to the ants, as they prevent the beetles from running away. The sticky secretion of the Aphides must also be injurious, if we may judge by the eagerness with which they allow ants to remove it; and we must therefore suppose that it also is due in large measure to selection by the ants.

The structural growths, which in many flowers necessitate the visits of special kinds of insects, or birds, to fertilise them, are also probably due to extrinsic selection; for it is very doubtful whether they are useful to the plants. No doubt the secretion of honey, the bright corolla, as well as all the devices by which plants prevent the visits of non-flying insects, or by which they entrap small flies, are due to the action of natural selection; for all of them are useful to the plant. But it cannot be of any importance to a plant that it should be visited only by a particular species of humble bee, or that it should be fertilised at night and not in the day; and we know that those plants which have the most elaborate apparatuses for securing cross-fertilisation by certain insects, are uniformly rare; and have not therefore been a success from their own point of view. Also, if it be good for a plant to have its flowers fertilised by pollen from some other plant,

then it is evident that the grouping of flowers into heads or spikes must be injurious, because it almost insures that the flowers shall be fertilised by pollen from other flowers of the same inflorescence; which Darwin says does little or no good; and yet capitate flowers are abundant.

Indeed, some of the variations which have taken place are decidedly injurious to the plant, such as the reduction of stigmatic surface in the Orchids, the abortion of one half of each anther in Salvia, and the asexual condition of the ray-florets in some of the *Compositæ*, and in the outer florets of the Guelder Rose. As a matter of fact, we find that in some cases these metamorphosed flowers are not sufficient to preserve the species from destruction, and have been supplemented by others which have special means for self-fertilisation. If cross-fertilisation were all that is wanted, the simple device of dichogamy—that is, the maturing of different parts of a flower at different times—would have answered every purpose.

But it is evident that capitate flowers are useful to the insects which visit them; for that arrangement enables the insects to collect a large quantity of honey, or pollen, with the least amount of trouble. And it is also evident that it is very advantageous to a humble bee to have a number of flowers in which the honey is so locked up that only she or her friends can extract it; and we may reasonably suppose that insects, finding honey sweet, began unconsciously to cultivate the plants. If a bee found that certain blue flowers always had more honey in them than yellow or white ones, she would certainly visit the blue flowers first. She would not know

why there was more honey in the blue flowers. She would never think that it was due to the difficulty other insects found in extracting it; but she would always visit them and expect a feast. We may readily suppose that each particular bee had her favourite plants, which grew in the neighbourhood; and, as she would constantly visit them in preference to others, it is easy to see how they might become isolated. In this way humble bees have caused long nectaries to grow, and the lips of the snap-dragon to shut; while moths have caused some flowers to keep closed all day, and so reserve their honey; and others to give out scent only at night.

The great number and abundance of plants whose inconspicuous gamopetalous flowers show that they were formerly visited by insects, although now fertilised by other means, is a sufficient proof that they have suffered no harm by the cessation of insect visits. They are the abandoned ones, thrown on one side, when a better class of goods offered; but they have not died out, nor even suffered any loss. Many botanists think that these flowers have retrograded since they were abandoned by insects; but there seems no reason why they should retrograde; and it is at least possible that some of them remain as they were, and simply mark the stages through which the more advanced flowers have passed.

By these means the most complicated flowers can be explained; for every modification which is useful to the plant is due to natural selection; and every modification which is useful to the insect-visitors is due to extrinsic selection.

SUMMARY AND RESULTS

Now let me sum up briefly the conclusions at which we have arrived.

The new Darwinism, like the old, has nothing to do with the origin of variations. It accepts them as facts existing in nature, and shows how these variations have been preserved, and how many of them have gradually developed into species, genera, families, and the higher groups. Whether variations be definite or indefinite, whether they be congenital or post-natal in origin, has nothing to do with Darwinism. It does not attempt to explain the fundamental cause or causes of variation; it only explains, first, the processes by which individuals become isolated, and therefore in a position to preserve any variations which may arise; and, secondly, how variations may be accumulated by the process of selection, and how progress in certain directions has been secured.

That part of the old Darwinism, known as natural selection, although occupying a most important part in the new Darwinism—for it is the great cause of progress—has been found inadequate to explain all the facts of organic evolution. The difficulties which cannot be overcome are three:—

1. Useless characters,
2. Incipient useful characters, and
3. The origination of divergent evolution.

These are now explained by isolation. The fourth objection—the existence of mutual sterility between different species—has been shown to be outside Darwinism altogether; which is a theory of the preservation and development of variations, and not of their origin.

It should be remembered that Darwin always said that natural selection was not, by itself, a sufficient explanation of organic evolution; and he mentioned isolation. But at that time the facts of distribution and of infertility were but little known; for it was not until after the publication of "The Origin of Species" that special attention was directed to these points, and their importance recognised.

It is natural selection, working with other forms of isolation, which has brought about the main progress of life. It gives a directive or determinate impulse to living organisms, and by its means life has advanced from the lowest Protozoa up to man. But, together with this progressive or determinate evolution, a large amount of indeterminate or indefinite evolution has been going on; and this has been the result of isolation working alone. We may liken the progress of organic evolution to the march of an army, which is continually throwing off numerous scouting parties, who penetrate into every nook and cranny, and leave nothing unexplored. The few that find roads, lead off part of the army after them; while the majority, who fail to do so, perish on their tracks, and are heard of no more. Natural selection preserves and intensifies adaptations, or utilitarian characters only; isolation preserves both utilitarian and non-utilitarian characters. Progress is due to the former, variety to the latter.

Now we rarely see a useful character which is not shared by many species; and, consequently, is of generic or even of higher value; while a very large number of specific characters are non-utilitarian. If we examine any large order of animals—that is, one

containing many species—we shall find that the ordinal characters, and those of the families into which it is divided, are of utilitarian value. But when we come to the smaller groups, the case will be different. A large proportion of the genera and almost all the species will be distinguished by characters, which, so far as we can see, are non-utilitarian, and could not, therefore, have been developed by natural selection. So it appears, after all, that natural selection is not so much a theory of the origin of species from varieties, as one of the origin of genera and the higher groups from species. Natural selection picks, as it were, here and there, one species out of many, and makes it the founder of a family. The rest—the vast majority—"have their day and cease to be." They vanish altogether from the board, without leaving any descendants behind them; although some linger longer than others.

It is then to natural selection that we owe progress, and it is chiefly to isolation that we owe variety. Without isolation all organic beings would have been nearly uniform, and all would have belonged to a single type, which would be the one best fitted for getting food and for propagating its race: a half-animal, half-vegetable, and a ruthless cannibal. This unhappy result was prevented by the first organisms being sexless, so that there was no inter-crossing, but each could develop independently. It is useful to contemplate what might have been, for we can then realise what the principle of isolation has saved us from; and we can the more readily recognise what an important part it has played in nature.

I must keep you a few minutes longer while I point

out where this leads us; for there is more than appears upon the surface.

We are accustomed to say, and I have repeated it many times in this lecture, that the characters developed by natural selection are utilitarian; that is, they are of use to the creature possessing them; while the characters due to isolation are for the most part non-utilitarian. Now we may at once grant that these latter characters are not, and never have been, of any use to their possessors. But are they of no use at all in the scheme of nature? Would man have been the same now if these non-utilitarian characters had never existed? Certainly not. It is the variety in nature that has excited man's curiosity, urged on his thirst for knowledge, and so induced him to study natural phenomena; while contemplation of the beauty seen in nature has stimulated his sluggish soul, and has developed his æsthetic and religious faculties.

Natural selection has, no doubt, developed that part of man's intellect which makes him cunning in devising means to ensnare his prey, and to get the better of his fellow man. But this, after all, is the form of intellect which man shares with the beasts; while the intellectual and spiritual qualities, which specially distinguish him, have not been called forth by natural selection. These are largely the result of contemplating the variety and beauty in nature; and, if natural selection has played an important part in gradually developing the body and mind of man, isolation has played a no less important part in developing his higher intellectual and spiritual faculties, and in teaching him to reverence and adore the Almighty Designer and Creator of all we see around us.

We are quite accustomed now to the idea that every structure in a plant or in an animal has a special object; else it would not be there. But it is generally supposed that this object must be a useful one to its possessor; for that is the teaching of the old Darwinism. Now, why should we limit ourselves to so narrow a view? We know that many structures exist which are not, and never have been, of use to their owner. Is it therefore necessary to believe that they are of no use at all? If we allow that the ultimate object of organic evolution is the development of man, not only physically, but also mentally and morally, I do not see how we can escape from the conclusion that all these so-called useless structures, all that give us beauty and variety, have been specially designed for his education.

Three hundred years ago geologists argued that fossils could not have been either simple freaks of nature, or the outcome of fermentation in the rocks —as had been previously supposed—because that would imply that the Creator had laid traps for man's intelligence, and had caused him to use his intellect for the purpose of leading him astray. Thirty years ago the same line of argument was used by zoologists with reference to rudimentary organs, and to the singular vagaries seen during the development of animals. It was urged that there must be some reason for these things, and that this reason could not have been to deceive man. In both cases the argument has been allowed; and why should it be disallowed in the present case? If beauty and variety *have* been a fruitful cause in the development of the special human characteristics: if they *have* been largely

instrumental in making him a being "of wise discourse, looking before and after," and if—so far as we know—they have been of no other use, why should we refuse to believe that this was the primary object for which they were designed? The only alternative is that man's higher development has been due to a lucky chance, and that evolution has no meaning.

This then, it seems to me, is the ultimate teaching of the new Darwinism; and I think you will all agree that we owe it a deep debt of gratitude, for lifting us out of the deadly region of utilitarianism into an altogether higher and purer air. But we must always remember that we could not have attained our present position, if we had not had the old Darwinism to climb with.

LECTURE III

DARWINISM IN HUMAN AFFAIRS

THE principle of selection, so important in the animal and vegetable kingdoms, plays also a very important part in human affairs; indeed it is universally present wherever there is competition. We select our dinners, we select our clothes, we select our books, we select our amusements; here it is not the inanimate objects which are competing, but their human producers. The one thing essential for the action of selection is competition; the one thing essential for selection to be cumulative is that the variations be capable of transmission to other individuals.

PHYSIOLOGICAL EVOLUTION

The enunciation of the principle of selection is simple. It is that, among two or more competing individuals, the worst adapted to the circumstances will be the first to succumb. This, you will say, sounds like a truism—and so no doubt it is—but, combined with the laws of inheritance and variation, it has brought about most important results. Now, it is necessary at the outset to get a clear idea of what selection can do, and what it cannot do; for this is a point very commonly misunderstood; and incorrect ideas on the subject have made some people reject the principle altogether.

If there are a number of competing individuals, differing slightly from each other, and if

> No compound of this earthly ball
> Is like another all in all,

then the principle of selection will, to a large extent, determine which of those individuals shall live to propagate its kind, and which shall die out. But selection has no power if the individuals are not competing; and it is in no way concerned with the origin of the differences; the differences must be there before selection can act. Those individuals which are best adapted to the circumstances will generally survive; and, by the law of inheritance, the chances are in favour of the offspring inheriting the difference that caused their parents to survive. If we take a sufficiently large number of cases, the probability of inheriting this difference becomes a certainty. If now these offspring again differ among themselves, in the same way that their parents differed, selection will again pick out those in which the difference—or variation, as it is usually called—is most favourable; and these selected individuals will, in their turn, hand the variation down to their offspring in a better form than they originally received it. Selection, therefore, in combination with inheritance and variation, is cumulative in its action; but it is important to notice that it is cumulative only by selecting the best varieties of each generation; it is in no way a cause of the variations themselves.

An example will perhaps make my meaning clearer. Take the evolution of the eye. Suppose that, in a number of eyeless animals, the nerve of some portion

of the skin of one of them was slightly sensitive to light. This one, being able to distinguish day from night, would have an advantage over other individuals in escaping its enemies; and thereby it will be one of those that survive, on the principle of selection; and in the next generation there will be, by the law of inheritance, several individuals endowed with this same power of distinguishing day from night. These, in their turn, will survive; and at last all the individuals of the species will have the same faculty. This will be the result of selection; but selection did not make the nerve of the first individuals sensitive to light; and it is powerless to improve the nerves of the offspring; what it can do is to bring all the individuals up to the level of the best. If, however, an improved variety appears, the improvement is at once made permanent, and diffused through the species by the action of selection; but selection itself, I repeat, cannot originate nor improve.

How variations arise we are profoundly ignorant. To suppose, with the late G. H. Lewes and Dr Roux, that there is an intercellular struggle going on in the tissues, and consequently an intercellular selection that causes variations in the organs, is merely to shift the difficulty one step backward, and by no means to explain the cause of the variations. To say, as some do, that variations arise by chance, is only another way of saying we cannot explain them. To say that no two individuals, or no two cells, are exposed to exactly the same conditions, and that, therefore, variations must arise—or, in other words, that all variations are caused directly by external conditions—is to draw an inference of

which there is no proof, and one that is apparently contradicted by a large array of facts of different kinds. No doubt, external agencies have some direct effect in producing individual variation; but it is doubtful whether these variations are ever transmitted; and in almost every case it is impossible to feel sure that a variation, supposed to be due to direct external action, is not in reality due to crossing. It would be more than rash, it would be foolish, to assert that we can never know more than we do now on the subject; but we may safely say that at present we know next to nothing. When we try to investigate the cause, or causes, of variability, we are confronted with nothing but difficulties, and we have no firm ground from whence to meet them. But, starting from the ascertained fact, that numberless variations are constantly arising, we can explain by the principle of selection how these variations accumulate in different directions, until in time the most diverse organisms are produced.

It is well known that in countries like Europe, long settled by civilised man, the total number of individual animals remains about the same, although many are born each year. Evidently this is due to the fact that the district is only capable of furnishing food for a certain number; and when that number is reached no further increase can take place. It follows from this, that in each year as many individuals must die as are born; and, as most animals and plants produce annually more than two young, it also follows that during each year the number of individuals that must perish is greater than the number that can survive. As therefore the supply of food is limited, there must

always be a severe competition for existence; a struggle to survive at the expense of neighbours. The individuals that die will usually be those that are least adapted for living—either for procuring food, or for escaping enemies. Useful variations will tend to increase, and succeeding generations will diverge more and more from the original stock. This is due to what is called natural selection.

But there is another mode of selection, which also causes each succeeding generation to diverge more and more from the original stock. This is best seen in the artificial selection by man of the plants and animals he has domesticated. Artificial selection may be either methodical, as when a man endeavours, by breeding from chosen individuals, to modify a breed according to some predetermined standard; or it may be unmethodical, as when the owner of a flock simply kills off the worst or wildest individuals, without any thought of altering the breed. Unmethodical artificial selection is not confined to human actions, but is also found among plants and animals. The difference between natural and artificial selection must be clearly comprehended; for, as we shall see, both are important factors in the study of politics. Natural selection is a selection taking place, as it were, by and among the individuals themselves; it is an internal principle, and may be called *intrinsic selection*. Artificial selection, on the other hand, is an arbitrary selection from outside, and may be called *extrinsic selection*. The great variety we find among wild flowers is due to both forms of selection combined—the intrinsic selection among the plants themselves, and the extrinsic selection of the insects that fertilise them. In garden

flowers we see the result of methodical extrinsic selection by man.

Another point, very important for you to notice, is that the principle of selection does not necessarily lead to improvement. If we consider a limited district, such as an island, it is plain that, in course of time, if the conditions of life remain constant, an almost exact equilibrium between the different plants and animals inhabiting it will be attained. The principle of natural selection, always acting in the direction of perfect mutual adaptation, will in time bring this about; and then, as no further modifications would be beneficial, none will be preserved—the organisms will remain stationary. But this stationary character, you must observe, only obtains while the conditions of life remain the same; if any change takes place, the adaptations can no longer be perfect, and further modifications will be beneficial. If the change is such that the conditions of life become harder, then the organisms must become more specialised; they must, as we call it, progress. But if, on the other hand, the change is such that the conditions of life become easier, the effect will be just the opposite—the organs will become more generalised, and the organisms will degenerate. I will illustrate this by an example. Suppose an island to be inhabited by hares, which are preyed upon by sheep-dogs, and that an equilibrium in speed has been arrived at, by which both maintain their relative numbers. If now greyhounds are introduced, it is evident that the conditions of life with regard to the hares will be harder; and, as only the fleetest will escape, all the hares on the island, in the course of some generations, will become

fleeter—that is, more specialised. But suppose that, instead of greyhounds having been introduced, all the sheep-dogs were removed, so that there were no dogs at all on the island, it is evident that the conditions of life would now be easier; the fleet would have no advantage over the slow; and the whole race would, to some extent, lose their special characteristic of swiftness—they would degenerate.

Psychical Evolution

Under different circumstances, sometimes physical structure, sometimes mental characteristics, may have the greater importance. Sometimes the cunning of the fox, sometimes the speed of the antelope, may be more advantageous.

Among animals physical structure is usually of more importance than superior mental powers; but in human societies mental capacity gets more and more powerful as civilisation advances; and we must therefore be prepared, when passing from the lower animals to man, to find the principle of selection considerably modified. The principle remains the same, and its action remains the same; but with man it acts chiefly in another sphere—the sphere of mind. An additional complication also arises. Among the lower animals selection only acts through utility; that is, through those conditions which tend to the physical well-being of the individual. But man is essentially gregarious, held together by the bond of sympathy; sympathy is as necessary to him as utility; and consequently selection will act as powerfully through the one agent as through the other. By the action of selection through

utility, intelligence has been raised into intellect; by the action of selection through sympathy with our fellow men, the moral sense has been developed, and ethical systems formed; through our sympathy with nature, imagination has given birth to art; and our æsthetic faculties have been evolved through the necessity for amusement, caused by the restless activity of the human brain. Let me explain very briefly how the more important of these things have come about.

Either from transmission, or from early association, every man has a number of opinions, common to the nation and to the class in life to which he belongs, which we may call his inherited opinions; for, if not actually inherited, they are formed at so early a period of life that for our present purpose they may be considered as inherited. But, as his reasoning powers develop, these opinions are subject to variation. The variations may be owing to original ideas arising in his mind we know not how, like the variations of structure in animals; or they may be due to education, that is, to coming into contact with other minds, either personally or through books; and it must be noticed that, unlike structural variations, these mental variations may be produced at any time in a man's life, and may or may not remain constant. Physical transmission is not necessary; mental transmission from mind to mind diffuses a variation rapidly through all the individuals; and consequently it is not necessary for the action of selection that the originator of an improved mental variation should have any bodily offspring, or should be healthy and strong.

When mental variations compete with one another,

selection constantly acts on them through the agency either of utility or of sympathy. When some member of a tribe, who was more ingenious than the rest, invented or improved a weapon or a snare, he would be imitated by the whole tribe. The tribe that contained the most ingenious men would have better means of obtaining food, and of defending itself; it would therefore increase more rapidly than others, and the intellectual faculties of the dominant tribes would gradually improve by competition. The love of praise and the dread of blame would be developed by the power of sympathy. The members of a tribe would unite in praising conduct which seemed to be lucky or for the general good, and in blaming that which seemed to be unlucky or evil.[1] A man who sacrificed himself for the good of his tribe would excite by his example the wish for glory in others; and glory would in time ripen into the idea of duty. With expanding intellect, and greater experience, other and higher virtues, such as temperance and veracity, would become esteemed; and by the action of selection they would be more and more practised; and so the moral faculties would also gradually improve.

The principle of selection, we must remember, is everywhere present; we cannot escape from its action. Just as each particle of matter is constantly under the sway of gravitation, so each thought, as soon as it has left the brain of the thinker, comes under the sway of selection. Fortunately most thoughts are smothered at once; but a few, which are adapted to the surroundings, spread far and wide, become dominant, and bend the variable minds of men to

[1] Darwin.

them. The opinions that spread must be adapted to the spirit of the times; but it does not follow that they are necessarily progressive; it is unhappily true that retrogressive opinions have frequently become dominant; but, in the long run, if competition continue, and sufficient time be allowed, we may expect that progressive opinions will prevail. Carlyle truly remarks that "everything goes by wager of battle in this world; strength, well understood, is the measure of all worth. Give a thing time; if it can succeed it is a right thing."

SELECTION IN POLITICS

In politics we have the principle of selection personified in the government which selects one set of opinions and makes it rule over the others; and this gives us the key to the science of politics. I will explain. Montesquieu divided all governments into (1) Republics, in which the whole or a part of the people have the supreme power; (2) Monarchies, in which a single person governs by fixed and established laws; and (3) Despotisms, in which a single person directs everything according to his own will and caprice. This classification is crude; but it will serve my present purpose. In a democratic republic, with a free press and universal suffrage, we have the type of government by intrinsic selection; everyone may freely express his opinion; and that set of opinions which can secure the greatest number of adherents wins the day. In an hereditary despotism, where, owing to accident of birth, one individual has power to force his private opinions on the rest of the nation, we have the type of government by extrinsic

selection. Between the two are many intermediate forms—the aristocratic republic, the limited monarchy, the absolute monarchy, and the elective despotism, in numberless variety.

Now we learn from biology that two things are necessary for securing the continued existence of a group of animals; one is strength to resist enemies, the other flexibility of organisation, the power of adaptation to changing circumstances. I use the word strength in a wide sense; there is strength in isolation, as well as in combination; and there is strength in insignificance. If a tribe of lions were to invade a country, the mice would not suffer much; but the deer and oxen might be exterminated. But strength alone is not sufficient; flexibility of organisation is perhaps even more important. If, when the conditions of existence are changing, a group of animals has an organisation so inflexible as to be incapable of variation—and animals are very differently constituted in this respect—it will necessarily be supplanted by some other group of a more yielding nature; and the same holds true with nations; but here the changes in the conditions of existence are to a large extent mental. If mere strength were all that was required for the preservation of a nation, then autocratic government would be the best; for government by discussion is not favourable for conducting war; but strength alone will not suffice; a flexible organisation, as Burke taught long ago, is equally necessary for duration of life.

How then is a nation to secure a flexible constitution? Evidently by adopting some form of government by intrinsic selection, which can respond readily

to any change in national feeling. A despot, no matter how benevolent his intentions may be, has no sufficient means of finding out the wishes of the people over whom he rules; for how could he "expose himself to feel what wretches feel?"[1] and, if he could ascertain their wishes, he would have great difficulty in carrying them into effect, as he must always rule through an army, which might not like the changes. Unless intrinsic selection is thoroughly carried out, there must always be the danger of the governing body misunderstanding the desires of the majority. In Sir G. C. Lewis' "Dialogue on the Best Form of Government," Aristocraticus is made to say: "The Corn Laws were part of a policy which was established on patriotic grounds, and which was at one time sincerely believed, even by enlightened men, to be beneficial to the entire community." Democraticus ought to have answered: "In this case, as in many others, the 'ignorant many' have proved wiser than the 'enlightened few.'" Despotism may succeed very well for a time; and is indeed indispensable in the earlier and more warlike stages of human society; but, when the conditions of existence change, all despotisms, being unable to accommodate themselves to the changes, must sooner or later succumb, either to internal or external pressure: they will pass away, and will be known only as political fossils of strange and uncouth form. And what is true of nations is true of all human institutions and organisations—a flexible constitution is necessary for continued existence.

Man must always have been a social animal; and his most powerful desires in early times would be the

[1] King Lear.

preservation of the common property of the tribe, and the spoliation of neighbouring tribes. To accomplish this the members of a tribe must work together; obedience is of the highest value; quarrelsome tribes could not cohere; every individual must work for the same end; the tribe is an army, and a chief would be naturally selected by consent of the majority. No division of power could be allowed; the chief must not have one opinion, the priest another; the priest and the chief must be one.[1] In time the desire for private property would arise; this would produce custom within the tribe; custom would develop into law; and this again would give rise to the desire for individual liberty of action. But, while it was war *à outrance*, this desire must be suppressed on pain of extermination; for liberty of action is not compatible with military superiority. Under these circumstances a despotism is the best form of government; variations in opinion are dangerous, and must be stamped out. The main virtues are courage, strength, and obedience; but these would nourish the moral qualities of truthfulness, mercy, and self-denial. As the desire for accumulating wealth grew stronger, the rising spirit of industry, and consequent increasing dislike to a military life, would favour the formation of a standing army for the maintenance of the power of the despot; and the tribe, now swollen to a nation, would become locked in an inflexible rule.

But in certain favoured places, where tribes were isolated, and consequently where protection against other tribes was not of such paramount importance, the desire for personal liberty would increase more rapidly; despotism would be enfeebled, or perhaps

[1] Bagehot.

would never arise; and the government would be carried on by discussion. These naturally protected districts might be backward in the art of war; but they would contain the germs of a principle destined to overthrow despotism and ensure the progress of mankind. Nations inhabiting rich, warm countries, which produced abundance of food, would be envied by their neighbours; and consequently they could never afford to give up despotism; for if they did so they would certainly be conquered by foreign nations whose customs they abhorred. But nations living in the bleak north, on land of which no one wished to deprive them, would develop government by discussion; the struggle for life against unkindly nature would strengthen the body; and government by discussion would invigorate the intellect. In warmer climes man gets physically and mentally enervated; and living under a despotism he becomes intellectually listless. So the northern nations would be constantly breaking in upon the southern nations, sometimes even conquering them, changing, perhaps, under the new conditions to despotisms, and then themselves degenerating. On the other hand, the better organisation for war given by despotism, and the greater population of the rich countries, might sometimes enable the southern nations to overrun for a time the northern ones; and so numerous complications would arise, not by chance, but by the operation of divine laws.

It follows from these considerations that, by the principle of selection, an isolated nation will develop government by discussion; but if, either by the approach of other nations, or by improvements in the means of communication, the isolation should

cease, the nation will either become itself a despotism, or it may be conquered by a despotism. In all despotisms extrinsic selection will check or destroy variation; and, just as an animal with an unyielding organisation remains unprogressive, and always liable to extinction when the conditions of existence change, so a despotic state may advance to a certain point, and must then remain stationary; while a nation with representative government will be highly variable; and will continue to progress, if it remains unconquered.

Such is an outline of a theory of politics as deduced from the principle of selection. Turgot was, perhaps, the first to demonstrate that history is not a series of cycles, but a single continuous progression; and, if Hume had known the principle of selection, he would, I think, have founded a science of politics; history would have been clear to him, instead of being, to use his own expression, "an inscrutable enigma." But it is to the late Mr Walter Bagehot that we owe the enunciation of the fertile principle, that discussion is to ideas what the struggle for existence is to corporeal entities; and that the best ideas are naturally selected under a government by discussion: you will find it ably developed in his "Physics and Politics."[1] It is not my place to apply this theory to the facts of history. I merely bring it forward to show you how politics and political history can be explained by the principle of selection. No doubt the evolution of society by means of this principle has always been going on; but it has been going on unconsciously; we are now conscious of it, and hope, by the intro-

[1] International Scientific Series, vol. ii.

duction of methodical intrinsic selection, hitherto unknown, to direct its movements. We stand at the turning point of a long series of ages; for, just as man is distinguished from the lower animals by the possession of self-consciousness, so are the times before us to be distinguished from the times gone past. A new light has fallen upon us; and that light has come from the study of biology. "I question," says Professor Jevons, "whether any scientific works which have appeared since the 'Principia' of Newton, are comparable in importance with those of Darwin and Spencer, revolutionising as they do all our views of the origin of bodily, mental, moral, and social phenomena."[1] And Mr Leslie Stephen also says, "Mr Darwin's observations upon breeds of pigeons have had a reaction upon the structure of European society."[2] As the atomic theory could never have been arrived at by a study of the complicated organic molecule, neither could a knowledge of the principle of selection have been arrived at by a study of the complicated phenomena of sociology; it was in biology alone that it could be detected; and it is to biology that we must turn for the proofs of its power.

THE SOCIAL ORGANISM THEORY

But there is another theory of sociology which I must not pass over, as it also is supposed to be founded on biological data. The curious analogy that undoubtedly exists between a state—or social organism, as it has been called—and an animal, or individual

[1] "Principles of Science."

[2] "History of English Thought in the Eighteenth Century."

organism, has been commented on by many writers ever since the days of Plato and Aristotle. The tissues of which an animal is built up are composed of cells, or units of organisation; and these units of organisation are supposed to represent the individuals which compose the state or nation. As the cells constantly die and are as constantly replaced, so do the individual human beings, while the nation lives on. A nation exhibits the phenomena of growth, structure, and function, like those of an individual body[1]; and in development both pass through changes which are not permanent. The governing or controlling power is supposed to represent the nervous system; the trading or distributing body to represent the vascular system; and so on. On the strength of this analogy many inferences have been drawn. The heart has been likened to the metropolis; and an overgrown metropolis is therefore said to be a disease. Because all parts of the body obey the mandates of the brain, imperialism, or at any rate centralization, has been advocated as the best form of government. As individuals have a limited period of existence, so also must it be with nations. This is the leading idea of Vico's "Scienza Nuova;" and we see it again in Lord Macaulay's celebrated New Zealander sitting on the ruins of London Bridge.

But the analogy is incomplete and misleading. Human beings are not so different from one another as are the various cells of which one of the higher animals is built up; and nothing can make them so different. In these animals each cell can only play its own part; but we know from experience that in a

[1] Herbert Spencer.

community a man can pass from a working member to be a controlling member, and often acts as efficiently as if he had always been a controlling member. Sancho Panza truly says, "As to governing well, the main point, in my mind, is to make a good beginning; and, that being done, who knows but that by the time I have been fifteen days a governor, my fingers may get so nimble in the office that they will tickle it off better than the drudgery I was bred to in the field." Indeed, it would not be difficult to find in this analogy as many discrepancies as likenesses. What, for instance, in the organisation of an animal answers to the professions of theology, medicine, or law? What to prisons and reformatories? How is it that, in the individual, one cell does not try to usurp the functions of another cell? How is it that one organ is never of opinion that another organ has too much power? How is it that one cell does not loaf on the industry of others, but each does its own work honestly? It is of no use to say that the social organism is young, and that all these things will disappear in time; because then we should have to compare the social organism of to-day with an individual lower in the scale than any now existing; and all the beautiful analogies of nerves and vessels would disappear. The fallacy of this analogy, as a guide to political science, is indeed so apparent, when we compare a highly differentiated nation like India with the much less differentiated one of the United States of America, that I am surprised it should have been adopted by the writer of an article on the "Science of History" in the *Westminster Review* for January, 1881. This writer proceeds to investigate what he calls the "physiology of history"

under numerous heads, in which he thinks he has dissected and examined a "social organism" as a biologist would dissect and examine an animal; but, in my opinion, he has only given new names to old things, and has not advanced our knowledge.

The term "social organism" is not, in fact, a happy one, because it is misleading. What is meant to be understood by this term is, not so much a single organism, as a number of groups of organisms, each group occupying a separate locality, and differing from one of the organs that make up an individual, in that it is more or less self-supporting, and capable of forming a new "social organism." The social groups, or communities, as I should prefer to call them, more nearly resemble what we call species; while the nation, which may consist of one or more communities, might represent the political genus, and might include extinct as well as existing communities. For example, the various communities of England in the fifteenth century may be said to be extinct, and to be represented by the communities of England, the United States, and the Colonies at the present day. It is the business of the science of history to explain why those communities became extinct, and how the present ones were developed; and, as a help towards a scientific solution of the problem, I may point out that the action of selection on each individual, through his external physical surroundings, is the chief determinant of the character of the community; whether, for instance, it shall be commercial, agricultural, pastoral, or manufacturing; as was dimly seen by Montesquieu and Buckle. The action of selection on society at large determines the course of politics and

the spread of religious opinions. National character is due to both; together with the inherited effects of selection on former generations.

IS A SCIENCE OF HISTORY POSSIBLE?

But, you may say, some of the best historians have denied the possibility of a science of history. That is true; but those historians have not studied biology; and without a knowledge of biology it is impossible to construct a science of history. Let us examine these objections. Mr Freeman says that there can be no science of history, because we cannot be sure of our data. No doubt the evidence on which history rests is often confused and contradictory, and has sometimes been intentionally falsified; but the main facts are certain enough. And are not the facts often confused and contradictory in natural science? Have we not even falsifications in nature? The resemblance of a whale to a fish deceived even the great Linnæus; and the resemblance of the eye of the cuttle-fish to that of a vertebrate has misled later naturalists. The inversion of strata has often misled geologists; and all the phenomena of mimetic resemblances are in a sense falsifications. Intentional deception may be more difficult to detect; but, if it had not been possible to detect forgeries, we should not know that any had been committed. That the science of history is difficult will be allowed; but that does not make it impossible.

Mr Froude believes that there can be no science of history, because human beings have free-will. He says, "When natural causes are liable to be set aside and neutralised by what is called volition, the word science

is out of place." But in all sciences one cause may be neutralised by another; and volition is, as we have seen, the cause of mental variations, without which selection could not act. A science of history can exist without a knowledge of the origin of ideas; although, of course, without that knowledge it will be incomplete. This, indeed, was pointed out by Kant, a hundred years ago, although he could not explain it. "Whatsoever difference," he says, "there may be in our notions of freedom of the will, metaphysically considered, it is evident that the manifestations of that will, viz., human actions, are as much under the control of universal laws of nature as any other physical phenomena."[1] We might as well say that there can be no science of biology, because structural variation obeys no known law. No doubt it makes prediction very difficult, or even impossible, except in a general way; but it does not follow that there is no science of biology. The idea that the power of prediction is essential to a science was originated by Comte, as a corollary from the Positive Philosophy; and it has been widely accepted by unscientific men without much consideration. But, if this be taken as a test, science will be reduced to those portions of astronomy and physics which are capable of being treated deductively by mathematical analysis. The astronomer cannot predict the appearance of comets; the physicist cannot predict the rate of expansion by heat of an untried substance; the chemist cannot predict the properties of a new compound; the geologists cannot predict the future physical geography of the earth; but, as Mr J. S. Mill says, "We must remember that a degree of

[1] "Idea of Universal History," translated by De Quincey.

knowledge, far short of the power of prediction, is often of much practical value. There may be great power of influencing phenomena, with a very imperfect knowledge of the causes by which they are in any given instance determined. It is enough that we know that certain means have a tendency to produce a certain effect, and that others have a tendency to frustrate it."[1] If this is the amount of knowledge required to form a science, then certainly there is a science of history.

Much as I admire Professor Jevons' "Principles of Science," I must object to this one sentence—"A science of history, in the true sense of the term, is an absurd notion." I object to it, because by many people it will be taken to imply that a science of history is impossible. Such however is not, I think, the meaning of Professor Jevons; because he continues as follows:—"A nation is not a mere sum of individuals whom we can treat by the method of averages; it is an organic whole, held together by ties of infinite complexity. Each individual acts and reacts upon his own smaller or greater circle of friends; and those who acquire a public position exert an influence on much larger sections of the nation. There will always be a few great leaders of exceptional genius or opportunities, the unaccountable phases of whose opinions and inclinations sway the whole body, even when they are least aware of it. From time to time arise critical positions, battles, delicate negotiations, internal disturbances, in which the slightest incidents may profoundly change the course of history. A rainy day may hinder a forced march, and change the course

[1] "System of Logic."

of a campaign; a few injudicious words in a despatch may irritate the national pride. The accidental discharge of a gun may precipitate a collision, the effect of which may last for centuries. It is said that the history of Europe at one moment depended upon the question whether the look-out man on Nelson's vessel would or would not descry a ship of Napoleon's expedition to Egypt, which was passing not far off. In human affairs, then, the smallest effects may produce the greatest results; and in such circumstances the real application of scientific methods is out of the question."

I have given this long quotation, because I wish you to have the case fairly put before you. From it I gather that Professor Jevons' words are directed against the application to history of the doctrine of averages as advocated by Buckle; and he means, I think, that human affairs are too complicated to allow of results being often predicted; but this, as I have already said, is very different from denying the possibility of a science of history. If I throw a glass bottle on the ground, I can safely predict that it will be broken; but, notwithstanding the perfection of the science of mechanics, no man can predict into how many pieces it will be broken. And the same with history: we may safely predict that the Turk will not rule long in Europe, although it is impossible to predict in detail the events which will turn him out. As a case of a verified prediction in history, I may remind you that, at the commencement of the campaign of 1806 between France and Prussia, Jomini predicted that a decisive battle would be fought in the neighbourhood of Jena; and he was right. No biologist would

deny that great results may proceed from small causes; but all would certainly object to the deduction that therefore "a science of biology in the true sense of the term is an absurd notion." This discussion is, I hope, sufficient to show that, although the science of history is exceedingly difficult and complicated, it differs only in degree, not in kind, from the other inductive sciences. It is not necessary that it should be able to predict what is going to take place, but only to explain what has taken place.

You ask, Why am I so interested in a science of history? What have I to do with it? I answer: History is a part of sociology, and sociology is but a branch of biology. It is the natural history of man, and must be approached through the study of biology. That sociology is dependent on biology was first definitely suggested by Auguste Comte; but he wrote before the principal laws of biology had been discovered; he thought that the idea of continuous progress was peculiar to sociology; and consequently he failed to see the true connection between the two. It is to Mr Herbert Spencer that we are indebted for making this connection clear. He has shown that a preliminary study of biology is essential to the student of sociology, "partly as familiarising the mind with the cardinal idea of continuity, complexity, and contingency of causation in clearer and more various ways than do other concrete sciences, and partly as familiarising the mind with the cardinal idea of fructifying causation (*i.e.*, cumulative action), which the other concrete sciences do not present at all,"[1] but which is common to biology and sociology. He points out that

[1] "Study of Sociology," International Scientific Series.

"the human being is at once the terminal problem of biology and the initial factor of sociology." As man is modifiable by surrounding conditions, it is necessary that the sociologist should acquaint himself with the laws of modification to which organised beings in general conform; and he concludes by saying that "the effect to be looked for from the study of biology is that of giving strength and clearness to convictions otherwise feeble and vague."

With ethical evolution Darwinism comes to an end. The process is now carried on by the higher stages of religious development, in which competition and selection have no place; for in heaven there are many mansions and room for all.

LECTURE IV

THE NEW LAMARCKISM

"LAMARCK ascribed some of the evolutionary changes of structure to changes in the environment, some to the motions of organic beings, and others to both combined."[1] This theory was developed with great skill by Mr Herbert Spencer, in his "Principles of Biology" (1865); and he must be considered as the founder of the school of Neo-Lamarckians, whose doctrines have been, during the last thirty years, ably advocated by several of the leading naturalists in the United States of America, among whom the late Professor E. D. Cope took a conspicuous place. The Neo-Lamarckians do not deny the efficacy of the Darwinian principle of selection; but they give it a subordinate place, and rely chiefly on the Lamarckian theory of evolutionary changes just mentioned. It is their laudable wish to have a complete theory of organic evolution; but we must remember that Lamarckism only claims to explain variation, and leaves untouched the problem of transmission of characters. So that, even if true, it no more goes to the root of things than does Darwinism, and leaves us very little more advanced than before. Nevertheless, we should be thankful for any advance, so long as it is a sure one, and founded on a

[1] Prof. E. D. Cope in "Primary Factors of Organic Evolution," 1896, p. 518.

careful examination of all sides of the question. But, although the Neo-Lamarckians have shown a great deal of ingenuity in framing explanations of the working of their theory, they have done little or nothing towards verifying it. Indeed, Professor Cope, in the preface to his "Primary Factors of Organic Evolution," candidly acknowledges that his book is "constructive and not destructive;" by which he appears to mean that he has brought forward all the facts he knows that assist to build up his theory, and has left out all those which tend to destroy it. I therefore propose to examine the evidence advanced by the Neo-Lamarckians, and to try and find out what reliance can be placed upon it, so as to ascertain what place, if any, Lamarck's theory has in biological evolution. But, before doing this, it will be necessary to make some general observations which will clear the way, and, I hope, prevent obscurities in the argument.

That their physical surroundings must have some effect on plants and animals is obvious. Different foods furnish different substances, which must give rise to different chemical reactions in different organisms. Temperature, light, and moisture necessarily affect these chemical changes, and so affect the functions of the organs; and these changes in function may sometimes change the form of organs, especially by encouraging or checking growth. Also it is obvious that there are internal physiological forces—due to inheritance—in all living organisms, which tend to counteract the physical forces, so that the latter can never have their full effect. These are the common property of all theories of evolution; but we cannot use them as a safe foundation for deductive reasoning

until we can explain the phenomena of growth and inheritance.

So far as we can see at present, variation must be due, either to the action of the environment on the adult, or to the mixture of ancestral characters in the germ-cell (Amphimixis);[1] and the difference between Lamarckism and Weismannism is that the former assumes that the action of the environment is direct and more potent than the admixture of germs; while the latter assumes that the action of the environment is comparatively small, and acts indirectly through the reproductive cells.[2] Direct action of the environment is more powerful, and rapidly produces perceptible changes in parts of the body, giving rise to what are called *acquired characters;* while indirect action is slight; it makes no outward show, but influences the reproductive cells, and thus becomes congenital; but it is only through numerous repetitions that any obvious result is brought about. However, there is no real difference between the two; for acquired characters may also affect the reproductive

[1] The following classification of the different kinds of variations may assist the reader in getting clear ideas on the subject:—

1. *Blastogenesis* (Weismann). The variation originates in the germ-cells, giving rise to congenital characters
   (*a*) by Amphimixis, or sexual union; or
   (*b*) by the indirect action of the environment on the germ-cells.
2. *Somatogenesis* (Weismann). The variation originates in the body (Soma), and is transmitted to the germ-cells, giving rise to acquired characters
   (*a*) by Kinetogenesis (Cope), or use-inheritance; or
   (*b*) by Physiogenesis (Cope), or direct action of the environment on the organism.

[2] At first Weismann thought that the environment acted on the protozoa only, but he afterwards modified that view.

cells; and indeed they must do so before they can be transmitted. Hence all transmissible varieties must be congenital.

Many external forces are simultaneously in action; and it is probable that they would not influence all the individuals alike. Usually some one force would exert a greater influence on one individual, and another on another; when these influences reached the germ cells they would come into connection with numerous ancestral influences; and the result might be very different in different individuals. Occasionally some force, or set of forces, might be more potent than the others, and cause many individuals to vary in the same direction. In the first case we should have indefinite, in the second, definite variation; but I have already explained (Introduction, pp. 6-8) that there is no essential difference between them; and that indefinite often pass into definite variations.

Most variations take symmetrical relations on each side of the body; but this is due to the unknown laws of symmetrical growth, either serial or bilateral, and is a necessary part of the organisation of the animal. As Weismann says: "The evolution of the species of *Deilephila* shows that the evolution of the marking follows throughout a certain law; that it proceeds in all species in the same manner. All the species seem to steer towards the same point; and this gives the impression that there is an internal law of evolution, which, like an impelling force, determines the future phyletic modification of the species."

We see much the same thing in dreams. Something which has lately fixed the attention gets mixed up in our brain with ancient memories; but the

mixture is not incoherent, even during sleep. The results are generally absurd or impossible; but they are not incoherent. There is a method in the wildest dreams. The ideas are correlated by some unconscious working of the brain; so that dreams, like variations, seem to be more or less definite.

Lamarck assumed that the action of external conditions was direct and definite from the first, and that the variations so produced were capable of being transmitted to future generations; and it is these two points that the Neo-Lamarckians try to uphold. In my examination of them I will follow the classification and nomenclature of Professor Cope, who divides the causes of variation into two groups, which he calls Physiogenesis and Kinetogenesis respectively. In the former the modifications are due to physico-chemical causes, in the latter they are due to mechanical causes.

PHYSIOGENESIS

There is no *à priori* reason for supposing that variations caused by physiogenesis must be useful, for they are brought about by physical or chemical forces, quite beyond the control of the organism; and the future of variations so brought about must depend upon the action of natural selection. In kinetogenesis, however—which is the chief distinguishing point of Lamarck's theory—we have an explanation of adaptation, and therefore a competitor with natural selection. But before an organ can be used it must be there, and mechanical stimulus cannot possibly originate a new secretion; consequently kinetogenesis

—like natural selection—cannot be a cause of new variations, but can only increase them in certain directions. Lamarckians, therefore, must fall back on physiogenesis as the originator of all variations, and can use kinetogenesis as a developer only; and, until the Neo-Lamarckians can explain how variations are produced by physiogenesis, their theory is no more complete than Darwinism. I need hardly say that this explanation is not yet forthcoming.

Animals are constantly changing their environments, and at the same time they are constantly varying, so that the two must often occur together. But it does not follow that the first is the cause of the second. *Post hoc, ergo propter hoc* is not a safe method of reasoning. To test it we must look for other evidence.

Experimental Evidence

First we have the well-known experiments on the change of colours in butterflies, caused by keeping the chrysalids at different temperatures. There are also the changes of colour in birds, caused by feeding them on different foods; the changes of colour in flat-fish when exposed to the light; and the change of one species of the Phyllopod crustacean (*Artemia salina*) into another, by regulating the amount of salt in the water; and even its change into another genus (*Branchipus*), by gradually making the water fresh. At the same time it must be remembered that, in the vast majority of cases, we are quite unable to bring about any variation by changing the external conditions.

Of the examples quoted, the changes of colour due

to changes of food or of temperature are no doubt purely chemical, although we cannot at present follow them; and the development by light of coloured spots on the under surface of flat-fish belongs to the same category. The change from *Artemia salina* into *A. milhausenii* is very remarkable; but it is not difficult to imagine that an excess of salt in the water might prevent the growth of hairs. The change, however, of *Artemia* into *Branchipus* is most extraordinary; for the latter genus, in addition to other characters, has an additional abdominal segment. How the absence or presence of salt in the water could determine the presence or absence of an abdominal segment is incomprehensible; and it would be as well if Schmankewitsch's observations were confirmed by careful experiment; for there seems to be no reason why the ova of *Branchipus* should not have been introduced with the fresh water which killed *Artemia*.

The relation between the size of some mollusca and their environment, observed by Dr K. Semper, is probably due to the effects of confinement on their nutrition or respiration; and it can hardly be called variation in its proper sense. At any rate, it is not the development of anything new.

The remarkable colour-changes which take place in chrysalids, by which they assimilate their colour to that of their surroundings, is generally brought forward as a case of the direct action of the environment. In one sense this is true; but the result is not an acquired colour which may be handed down to another generation, as its whole importance lies in its being transitory and not transmitted; for the colour of one chrysalis would not do for all its descendants.

The capacity for changing colour is not due to the direct action of the environment, but has been gradually brought about by natural selection; for this change of colour only takes place once, and cannot be reversed. We can imagine that in the skin of the caterpillar or chrysalis there are a number of molecules, capable of being combined into different chemical compounds by the action of light, and that the nature of the combination will depend on the rapidity of the light vibrations; and that the colour of the resulting compound will resemble that of the surface which reflected the light. But this delicate apparatus must have been very slowly produced; and, as it is useful, we cannot doubt but that it has been developed by natural selection.

Experiments with plants have given such contradictory results in the hands of different experimenters, that they prove nothing;[1] and we must see whether a better case can be made from the observational evidence.

Observational Evidence

The action of climate is especially observable in plants; and many characters have been attributed to the direct action of a dry climate—such as spines, a hairy epidermis, coriaceous leaves, succulent leaves, large roots, bulbs, tubers, &c.[2] But many of these, perhaps all, can be also explained by natural selection, and furnish therefore unsatisfactory evidence. However, it is allowed that the direct action of climate

[1] See "Weismann on Heredity," English Edition, Essay vii.

[2] See the discussion by Dr Wallace and the Rev. G. Henslow in *Natural Science*, vol. v.

does cause changes in individual plants; the only doubt is whether these acquired characters can be transmitted. I shall presently adduce the case of *Veronica lycopodioides* as favourable to this view.

Thickening of the fur may possibly be caused by the greater health of some animals in cold climates; and this appears to have been rapidly accomplished by the Tasmanian opossum, turned out in the south of New Zealand. The loss of hair by dogs in hot countries is certainly due to ill-health.

On the other hand, there is plenty of evidence to show that variation may take place without any change in the environment, and therefore that it may have some other cause. I have already mentioned the Albatrosses, the Mutton-birds of the Kermadec Islands, and some land birds of New Zealand (see ante, pp. 87-90); and I will only give as another instance the ancient order of Trilobites. These animals fed on mud at the bottom of the sea, where they must have experienced very little change in their physical condition; yet they varied a great deal.

We also learn from palæontology that some species change, while others, living with them, remain constant. For example, the Ammonites were a very variable group; but the genera *Phylloceros* and *Lytoceros* remained almost unchanged from the Triassic period to the Cretaceous, although they lived with the others. We cannot explain this on the supposition that variation is entirely due to change in physical conditions.

In the land Planarian-worms we find that the lower surface is flatter than in the marine forms; and we might attribute this to the difference in environment, if it were not for the fact that no such change has taken

place in the land-leeches, nor in the land Nemertean-worms, which live with the land Planarians.

When fishes first made good their footing on land, it was by help of the air-bladder, which changed into a lung. But since then the climbing perch (*Anabas*) has become an air-breather, not by means of its air-bladder, but by a series of folded plates in a special cavity above the gills. In the land-crabs (*Gecarcinus*) the gills are small, and the greater part of the bronchial cavity is filled with air; while the cocoa-nut crab (*Birgus*), which belongs to another sub-order, has a special air-cavity, above the gills, which is lined with a lung. Now, if the physical action of the air is the cause of these structures, how is it that it has acted differently in different cases? If it produced a lung in the Amphibians and in *Birgus*, why did it not do so in *Anabas* and *Gecarcinus?* It seems more probable that these different organs were due to the natural selection of indefinite variations, and not to the direct action of the air.

While, therefore, we must allow that the direct action of external conditions has some effect, we find very little evidence in its favour. The *à priori* reasoning seems excellent, until we apply it in detail; and then it fails us. There is, however, ample evidence to show that variation is, to a large extent, independent of external agencies, and depends upon constitutional organisation. Evidently variation is the result of very complicated conditions, and is not to be explained by one overmastering principle. It must be the resultant of several forces which are nearly equally balanced.

KINETOGENESIS

Variations due to the movements of animals are here included; and I have already pointed out that it is a theory of the development of pre-existing variations only, and not one of their origin. Consequently the name kinetogenesis is incorrect and misleading, and, Mr Ball's term, "use-inheritance," [1] is more appropriate.

The effect of use on a muscle (hypertrophy) is known to everyone; and the result is no doubt due partly to excess of nourishment and partly to the induced state of the nerves; but this does not give characters of specific importance. Extra nourishment, or even mechanical stimulus, may increase the secreting power of cells; and so the shape of growing bones may be affected; but it is hard to see how kinetogenesis can apply to the joints of insects, which do not grow. Professor Cope, however, says that "all the form characters of the vertebrate skeleton, and, for that matter, of the hard parts of all animals, have been produced by muscular pressures and contractions, and the friction, strains and impacts due to these." [2] But he makes no attempt to explain the development of the defensive armour of turtles and many other animals; nor is it possible to explain by kinetogenesis the curious elongated strap-shaped roots of the mandibular teeth of the goose-beak whale (*Mesoplodon Layardi*). These are vestigial teeth, the roots of which, in this particular species, grow to so great a length that they impede the opening of the mouth, and appear to have no use. Also, according to Sir Samuel

[1] "The effects of Use and Disuse," by W. P. Ball (1890), p. 23.
[2] "Primary Factors of Organic Evolution," p. 467.

Baker, the African elephant invariably uses one tusk in preference to the other[1]; but this has produced no difference in size, except that the working tusk is smaller by wear. If it be objected that the law of symmetrical growth would prevent one tusk from growing more than the other, then we must also reject kinetogenesis as the explanation of the single tusk of the Narwhal, and of asymmetry in the skulls of the sperm-whale and the flounder.

To come now to the examples brought forward by the Neo-Lamarckians, the origin of the shelly plaits on the columella of some Gastropods has been attributed to kinetogenesis. But these can hardly have been caused, as asserted, by the dragging of the wrinkled mantle in and out of the aperture, because they are more simply explained as being due to secretion of shelly matter going on after the mantle was withdrawn, and therefore in a wrinkled condition—the plaits being formed in the wrinkles. It is true that the shape and ornamentation of shells depends largely on the shape of the mantle of the animal; and, if this is what is meant by kinetogenesis, we must all agree to it. But then the important question arises, What shaped the mantle?

The spiral form of the shell in many Gastropods and Cephalopods has been attributed to the weight of the shell making it incline to one side. But the spiral form is seen in some of the earliest pelagic Gastropods, as a delicate horny shell, the specific gravity of which could not have exceeded by much that of the tissues of the animal. It is a fact that the

[1] "The Nile Tributaries of Abyssinia," p. 361; and "Albert Nyanza," p. 172.

spiral form preceded the deposition of carbonate of lime; and it seems probable that this form proved to be less exposed to damage than the elongated tube, which preceded it.

Again, an attempt has been made to explain the impressed zone on the inner side of the whorls of some nautiloid shells as being due to the pressure of the outer whorl as it grew round the inner one. Professor Hyatt says, "That this tendency (towards closer coiling) is quite capable of producing the impressed zone can hardly be denied with any show of reason, since the characters tend to disappear in proportion as the pressure is relieved."[1] It is, no doubt, true that the impressed zone is caused by close coiling; but I cannot see how pressure could have anything to do with it. When a sponge grows round a piece of rock, is the shape of its under surface due to pressure? An inherited tendency to grow in a coil which is too close for the expanding diameter of the shell, would necessitate one whorl growing over another; and, if this inherited tendency was overcome, and the animal straightened, there would be no longer this necessity for growing over the last whorl. There is no reason to suppose that any pressure was exerted by the mantle; for there was always free space in other directions; and, of course, as the shell is deposited by the mantle, it could not cause pressure by itself.

The Neo-Lamarckians seem to think that, when any structure is shown to be a necessary portion of some apparatus, it must in some way, which they do not explain, have been developed by the movements of the animal. For example, Dr R. T. Jackson says that

[1] Quoted by Cope, l.c., p. 415.

the single adductor muscle, which is found in the embryo-stage of many bivalve molluscs, "does not seem homologous with any muscle in other classes of molluscs, and is probably developed from the mantle-muscles *as a consequence of the conditions of the case.*"[1] Many other instances could be quoted; but it would be useless to do so, as they all have the same defect; they assume as true the very point they wish to prove.

Another curious example of reasoning is given by Professor J. T. Cunningham, in the Introduction to his translation of Eimer's "Organic Evolution" (1890). He proposes the tongue of the woodpecker as a test case between the Neo-Lamarckians and the Neo-Darwinians. He begins by saying that it is admitted by Weismann's adherents that the size and shape of bones and muscles are, in the individual, modified by the use which is made of them; and they will probably admit that the modification is such as to facilitate the operations in which they are used. The Neo-Lamarckians, he continues, believe that these modifications were inherited in some degree, and so accumulated in a succession of generations; while the Neo-Darwinians deny that the use-modifications are inherited, but assume that another set of modifications, of the same kind, arose by congenital variation; and that the individuals that had these survived. This, he concludes, is very much like saying that the "Iliad" and "Odyssey" were not written by Homer, but by another man of the same name, who lived at the same time.

But the fallacy of Professor Cunningham's reasoning

[1] Quoted by Cope, l.c. p. 262. The italics are mine.

is obvious. No adherent of Weismann, and no Neo-Darwinian, would admit that the protruding of the tongue in the woodpecker could elongate the hyoid bones; nor could it make the tongue horny and pointed, nor could it make it barbed at the end. To suppose that the use of an organ will have some effect on it, is very different from assuming that it will completely alter some structures, and even originate new ones in connection with it. As I have already mentioned, the use of an organ cannot possibly originate anything new. If the elongation of the tongue and the barbs at the end are due to the action of protrusion, why have not the tongues of the Nuthatch (*Sitta*) and of the Tree-Creeper (*Certhia*) become elongated? And why has that of the Wryneck (*Yunx*) been elongated but not barbed? Also, why should the Ant-eater (*Myrmecophaga*) and the Australian *Echidna* have developed a long snout, instead of, like the Woodpeckers, curling the withdrawn tongue behind the skull? If these long snouts are the direct result of protruding the tongue, why has the same cause had such different effects in different animals?

I think that Mr W. H. Dall takes up a more logical position when he says, speaking of the hinge in bivalve shells, "Nature, through natural selection and physical stresses, has developed these cardinal processes which we know as teeth."[1]

But it is from the vertebrate skeleton that the Neo-Lamarckians take most of their examples. It is known that when joints in living animals are disarticulated, new surfaces are formed; bone is deposited and cartilage replaces the periosteum. Bone is deposited

[1] "American Journal of Science," vol. xxxviii. (1889), p. 542.

under the stimulus of moderate intermittent irritation; while under prolonged pressure it is removed. From these facts it is inferred that the vertebrate skeleton can be explained on mechanical principles. Professor Cope points out that the tongue-and-groove ankle-joint is found in nine different groups of the mammalia; and he says that "from the preceding facts I have inferred that in biologic evolution, as in ordinary mechanics, *identical causes produce identical results.*"[1] No doubt this ought to be true, if the skeleton is due to mechanical forces; but, when we proceed to test its truth, we find that Professor Cope's argument breaks down, and that he has selected his facts from those which he considers favourable to his hypothesis, and has omitted all others.

Compare, for example, the wing in a *Pterodactyle*, a bat, and a bird. Here we find three totally different structures performing the same function. The *Pterodactyle* has only one finger elongated, the bat has four fingers elongated, while the bird has none; yet all, according to the Lamarckians, have been produced by the same cause; and we get the nonsensical inference that identical causes produce different results. Also, in both the bat and the *Pterodactyle* the wing membrane must have preceded the peculiar movements of flying, and could not have been caused by them, as the Lamarckians suppose; for no animal would flap its fore limbs without any reason.

Professor Cope also says that the length of the legs of terrestrial mammals has increased with time, and that this is apparently due to use. In the majority it is increase of length in the foot, but in the Planti-

[1] l.c. p. 361. The italics are mine.

grades it is in the femur or tibia. "We can trace these to special uses," he says. "Those which receive the principal impact in progression are those which increase in length;" but the carpal and tarsal bones do not elongate like the others under the same stimulus, because of their phylogeny. In those Ungulates which live on hard dry land, the outer toes have been reduced, because they do not receive so many impacts as the central ones.

But if the legs of Ruminants have been lengthened by impact-stimulus, due to running over hard ground, how are we to account for the long legs of wading birds, of the Heron and the Stilt-plover (*Himantopus*), which only alight on soft, swampy ground? And I might also ask the Neo-Lamarckians to explain the long toes of the Jacana (*Parra*), which walks over floating leaves.

Again, compare the laboured flight of the Tropic-bird (*Phaëton*) with the graceful movements of the Frigate-bird (*Tachypetes*). I suppose that the former flaps its wings at least six times to one movement of the wings of the latter; and both birds fly all day long, and for most days of the year. Here we have a great contrast in the amount of work done by the joints and muscles of the wings; and yet no difference has been brought about in the shape of the glenoid cavity, nor in the head of the humerus. And, in spite of the more rapid movements in the Tropic-bird, its wing feathers are shorter than those of the Frigate-bird; so that, if more nourishment has been sent to them, it has had no effect. A similar comparison could be made between the Shear-waters (*Puffinus*) and the Oceanic Petrels (*Œstrelata*), and with the same results.

We shall find another simple set of conditions in the vertebral column; and we have in it an excellent illustration of the independent origin of similar structures. For in the earlier forms of *Elasmobranchii*, *Crossopterygii*, *Actinopterygii*, and *Amphibia*, the backbone consists of the notochord only, but in the later forms of each group we find it broken up into ossified vertebræ. These vertebræ must, therefore, in each case have originated independently.

It is easy to see that a hard, bony attachment for the muscles would be better than the softer sheath of the notochord; but in fishes, which use their bodies and tails in swimming, the hard, bony rod must have many flexible joints. Consequently, if ossification took place at all, vertebræ must be the result, or the fish would die. However, we have in the Lampreys, the Sturgeons, the *Holocephali*, and the *Dipnoi*, living fishes with notochords only; so it is evident that the development of vertebræ is not a necessity caused by the movements of the animal; and therefore the independent development of bony vertebræ in four different groups of animals cannot be taken as a proof that they were necessarily developed by mechanical movements. On the contrary, as the thoracic bony vertebræ develop in the young before the caudal vertebræ, we infer that calcareous deposits first began in that region where the movement was least. Given a tendency in some individuals to deposit lime-salts outside the notochord, we can see how natural selection might preserve those varieties which showed the best segmentation, as well as those which deposited no lime at all. But Kinetogenesis requires that the deposition as well as the disposition of lime-salts should be caused by the

movements of the notochord, and gives no explanation why, of two notochords with approximately equal movements, one should form long vertebræ and the other should not do so.

Passing now to the shape of the centra of the vertebræ, Professor Cope says "there can be no doubt but that the ball-and-socket vertebral articulation has been produced by constant flexures of the column in all directions, as has been suggested by Marsh."[1] And Professor Marsh says that the saddle-shaped articulation in the vertebræ of birds has been caused by vertical movement; his reason for this opinion being that the third cervical vertebra in *Ichthyornis* is of an intermediate form between the cup and the saddle-shaped vertebra. Such a "modification of the vertebræ," he says, "would naturally appear first where the neck had most motion, viz.: in the anterior cervicals, and gradually would be extended down the neck, and on to the sacrum, if the same flexure were continued."[2]

But, if we test this hypothesis by Professor Cope's rule—that the same mechanical cause must always produce the same effect—we find it to fail; for fishes have not developed a vertebral centrum adapted for lateral movements, although they always move their tails laterally and never vertically. Also cetaceans have not developed saddle-shaped centra, although the movement of their tails is more strictly vertical and far more vigorous than the movement in the necks of birds. Again, why should crocodiles have developed ball-and-socket articulations in their dorsal vertebræ, while the Dinosaurians did not do so?

[1] "Primary Factors of Organic Evolution," p. 304.
[2] "Odontornithes," p. 181 (1880).

Certainly this could not have been due to greater movement in the crocodiles.

Again, the lengthening of the neck is sometimes, as in the swan, due to an increase in the number of vertebræ; while in other cases, as in the giraffe, the number of vertebræ is not increased, but each one is greatly elongated. How could the same mechanical cause have brought about such different results?

There is a close resemblance between the fish-like body of the Dolphin and that of the *Ichthyosaurus*. Both have the same elongated head and short neck, so admirably adapted for rapid movement through the water. But we cannot suppose that the head has been elongated and the neck shortened by the same process of holding them steady; for the same mechanical cause could not have produced opposite effects.

These examples are sufficient to show that an impartial application of Professor Cope's rule lands him in a multitude of difficulties; and I will now give an example of another class of cases, which at first sight seem to prove Kinetogenesis, but which, on closer examination, are found to be capable of other explanations, and therefore are not conclusive.

The asymmetry in the skull of the half-lop rabbit was adduced by Mr Darwin as evidence of change in form due to different stresses on each side of the head;[1] and this explanation certainly appears plausible. But the asymmetry of the skull may be the cause of the difference in the ears; or it may be that both ears and skull are due to the same cause, viz., a failure in symmetrical growth, which was taken

[1] "Animals and Plants under Domestication," 2d edition, vol. i., p. 124.

advantage of by the breeder, and fixed by artificial selection. The last seems to me the most probable hypothesis of the three; for, if the asymmetrical skull was caused by asymmetry in the ears, we still have to look to a failure in the law of symmetrical growth to account for the ears. And if, in any case, we must assume that a failure in the law of symmetrical growth was the original cause, there seems no reason why the failure should not have applied to both ears and skull simultaneously.

There is, however, one case in which Kinetogenesis seems to be the best explanation of the facts.

The asymmetrical skull of the flat-fishes, with the two eyes on one side, seems to be due to efforts made by the young animal to twist the lower eye upwards when it lies on its side.[1] The upper side with the eyes differs in different genera—it is the right side in the Sole (*Solea*), and the left side in the Turbot (*Rhombus*). Generally it is the same in every species of a genus; but, with the Flounders (*Pleuronectes*), although the eyes are generally on the right side, they are sometimes found on the left; and this is the case with different individuals of the same species; so that the only thing constant is the asymmetry of the skull. Darwin says "Schiödte believes, in opposition to some other naturalists, that the *Pleuronectidæ* are not quite symmetrical, even in the embryo; and, if this be so, we could understand how it is that certain species, whilst young, habitually fall over and rest on the left side, and other species on the right side." Dr Wallace, however, explains

[1] Darwin, "Origin of Species," 6th edition, p. 186. Wallace, "Darwinism," 2d edition, p. 129.

this as due to natural selection. He says that "those individuals who had this faculty (of twisting the eye) in the greatest degree were among those that survived," and that "it is not the change produced by the *effort* that is inherited, but the favourable constitution which renders extreme effort unnecessary, and causes its possessors to survive, while those less favourably constituted, and who have therefore to use greater effort, succumb."[1] This explanation is not satisfactory to me, because, if it were true, *Pleuronectes* ought to have its blind side always the same. The fact that it is not so seems to show that the asymmetry is, to some extent, due to individual effort in the young fish.

Among plants Kinetogenesis is not of much importance; but the Rev. G. Henslow has given an ingenious explanation of how all the modifications of flowers may, perhaps, be due to the irritation caused by the visits of insects.[2] This hypothesis has been adversely criticised by Dr Wallace[3]; but in my opinion Mr Henslow has made out a case that requires careful consideration. There are, certainly, floral modifications which cannot be explained by natural selection, as they are injurious or useless to the plants; but these are all useful to the insect-visitors, and may have been produced by extrinsic selection, as I have already explained. Mr Henslow has not brought forward a single case which cannot be explained by one or other kind of selection;

[1] *Nature*, vol. xl, pp. 619-620.

[2] The Origin of Floral Structures by Insect and other Agencies. 1888.

[3] *Natural Scienee*, vol. v. (1894), p. 178, and Mr Henslow's reply on p. 261.

and therefore he has not shown use-inheritance to be indispensable.

Also, passing by the apparent inconsistency of supposing that irritation has formed the receptacle by arresting the growth of the axis, while at the same time it has formed the flower by stimulating the growth of its foliar appendages, I find that he makes no attempt to explain the slow movements of the filaments and styles in many dichogamous flowers, by which the anthers or the stigmas take alternately the same position. He also allows that many structures are found in flowers which he cannot explain, except by calling in the principle of compensation of growth, a principle which, as I shall presently show, is a very doubtful one and certainly unproved.

But the strongest evidence against Mr Henslow is afforded by those flowers—such as Mignonette (*Reseda*)—which are constantly visited by insects, and yet show no marked result from their visits. If the modifications are due to mechanical irritation, they ought always to follow upon irritation, and to be proportional to it; but this does not seem to be the case. Nevertheless, the examples brought forward are very interesting, and are not to be altogether set aside, although Mr Henslow has evidently exaggerated their importance; and I shall consider them again when discussing the subject of heredity; when I will also mention the tendrils of *Ampelopsis*, which is, I think, Mr Henslow's strongest case.

DEGENERATE ORGANS

It is acknowledged that disuse of a muscle leads to degeneracy in the tissues, and to their gradual dwind-

ling away. Also that a deficiency in the nourishment supplied to an organ, especially during the period of growth, produces atrophy. It is further allowed that degenerate organs[1] are really the remains of organs which were fully developed in former generations. But how disused organs became vestiges, or disappeared altogether, is a moot point.

Three explanations have been proposed, viz., Natural Selection; Panmixia, or the cessation of selection; and Disuse-inheritance. Both Professor Lloyd Morgan[2] and Mr Romanes[3] have, however, independently pointed out that the idea of Panmixia being a cause of degenerate organs is founded on a misconception. The cessation of natural selection means the cessation of the destruction of the least fit. There would be no further progression, but no retrogression. If the disused organ is hurtful it will be removed by natural selection, but if it is harmless it will remain stationary, so far as panmixia is concerned. If an organ is to degenerate, the *minus* variations, as we may call them, must preponderate over the *plus* variations. But this cannot be brought about by panmixia, which means free inter-crossing between all the varieties that arise; it does not originate variations, but only averages them. Panmixia can no more lead organs downwards than it can lead them upwards; and as a cause of degeneration it must be eliminated.

Natural selection acts directly when the organ is harmful; but it is also supposed to act indirectly, through what has been called the principle of com-

[1] Often incorrectly called "rudimentary organs."

[2] "Animal Life and Intelligence," 2nd edition, p. 190 (1891).

[3] "Nature," vol. xli., p. 437 (1890).

pensation of growth; those individuals being eliminated which supply most nourishment to the useless organ. For example, in the case of blind animals living in caves; disuse of the eye might cause the pigment cells to degenerate and render the eye useless; and we may suppose that useless eyes, being a nuisance, might be removed by the direct action of natural selection. But, when we try to explain the disappearance, or partial disappearance, of the optic nerve, we have to fall back upon, either the principle of compensation of growth, or on that of disuse-inheritance. Now the principle of compensation of growth supposes that the *plus* variations are eliminated, not because they are *plus* variations, but because the individuals with *minus* variations have, as a consequence, some other and more important organ better developed, and so are preserved by natural selection. It is here assumed that the hypertrophy of one organ causes atrophy in another; an hypothesis which is not supported by sufficient evidence,[1] but has simply been taken for granted ever since the days of Goethe.

The fact that pruning stimulates dormant buds into growth furnishes no evidence that cutting off one part of a flower stimulates the growth of another part of the same flower; for the conditions are different; and I am not aware of any experiments having been recorded

[1] The few instances given by Darwin are all taken from domesticated animals or plants, and all are unsatisfactory, for they are complicated by the action of artificial selection. Dr Wallace suggests that the absence of the thumb in *Colobus* and in *Ateles* may be due to compensation of growth ("Darwinism," 2nd edition, p. 138, foot-note); but in 1866, Messrs Murie and Mivart had pointed out the improbability of this being the cause of the reduced fore-finger in the Potto (*Trans. Zool. Soc.*, vol. vii., p. 92).

showing that such is the case. Still less can we assume that the stimulation of the corolla would cause the atrophy of one particular stamen without affecting the rest. Nor can we apply the case of plants to that of animals, without danger of falling into serious error; for the whole plan of nutrition differs in the vegetable and animal kingdoms. In the former it is an involuntary act, and takes place whenever the conditions are favourable; in the latter it is voluntary. Animals only feed when they are hungry; and only in exceptional cases do they lay up large stores of reserve material in their bodies. When any extraordinary growth takes place in an animal, such as the renewal of a limb or a tail, there ought to be, as a consequence, the atrophy of some other organ, if the principle of compensation of growth be correct; but I know of no such case. Usually every animal receives abundant nourishment and to spare. In seasons of want all its organs suffer; and there seems to be no reason for thinking that unused organs lose more than others. We can hardly suppose that to the principle of compensation is due the degeneracy of intestinal worms, which live bathed in nourishment; but, if we reject the principle in this case, how can we apply it to explain the almost entire disappearance of the pelvic girdle in whales, or of the coracoid in Mammalia?

So that, in those cases where the direct action of natural selection fails us, the only explanation of degeneration appears to be disuse-inheritance; which assumes that unused organs deteriorate in each generation, and that they are transmitted to the next in a more deteriorated state than they were received.

The colours of the lower surface of flat fish seem

to me to offer a test case. The pale colour cannot be of any importance to the animal; and the presence of pigment cells in its skin cannot be a disadvantage. We know that these pigment cells are there, for they can be brought into prominence again by the action of light; but their degenerate state is now inherited; and it seems to me far more probable that this is due to disuse-inheritance than to natural selection.

The complete loss of the wings in the moa may, perhaps, be explained by the action of natural selection; as they may have been in the way when the bird was pushing its way through dense scrub; but we cannot refer to natural selection the suppression of the shoulder-girdle.[1] Neither can we call to our aid the principle of compensation of growth; for the birds lived on vegetable food, of which there is a great abundance in New Zealand; and it was owing to the abundance of food that they attained to such a great size. Even if we suppose that times of scarcity might arise, and the principle of compensation be called into action, we should expect that the deficiency would be made up, not by the suppression of small bones like the scapulo-coracoid, but by the reduction in thickness of the legs. For the thickness of the legs of the moas was of no advantage to them. On the contrary, it was distinctly a disadvantage; and would never have occurred if the birds had had any enemies from whom they tried to escape. Hence we are driven to disuse-inheritance for an explanation.

[1] The genera *Dinornis* and *Anomalornis* still retain the scapulo-coracoid, but in *Euryapteryx*, *Meionornis*, and *Pachyornis* that bone has entirely disappeared. The wing is absent in all.

INHERITANCE OF VARIATIONS

That congenital variations can be, and usually are, transmitted is acknowledged by all. The doubt is whether post-natally acquired characters can be transmitted; that is, characters acquired through the direct action of external conditions. I have already remarked that the difference between direct and indirect action of the environment is one of degree only; if, therefore, congenital variations are due to the indirect action of the environment, it follows that the transmission of quickly-acquired characters is not impossible, although it is very improbable that such should take place.

The reason for this improbability lies in the fact that, as Dr F. Galton has shown, the "heredity-power" of the actual parents is small, when compared with the accumulated heredity-power of the ancestors, stored up in the germ-cells.[1] So that any sudden change, even on the supposition that this change has affected the germ-cells, has the whole force of ancestral heredity-power arraigned against it.

The chief difficulty in investigating the problem of the transmission of acquired characters is the difficulty of determining what *are* acquired characters. Mutilations, no doubt, are acquired characters; but, as they are unlike ordinary specific characters, they have been, very rightly, objected to, as affording no trustworthy evidence. Nevertheless, the reported occasional inheritance of mutilations is always brought forward by the Neo-Lamarckians as a part of their argument. There appear to be several well-authenticated cases on

[1] "Proceedings of the Royal Society," vol. xx., p. 394.

record of the transmission of mutilations; but, as they are very exceptional and highly improbable, the evidence for them ought to be closely scrutinised. It seems to me that this evidence presents a very suspicious feature. If the effects of a mutilation were, in some way, passed on to the germ-cells—and this must be done before the mutilation could be transmitted to the next generation—the principle of symmetrical growth would come into play, and we ought to find the inherited mutilation equally developed on both sides. This, however, is rarely the case; and it shakes my confidence in all the recorded observations.

Professor Cope gets over the difficulty in a characteristic fashion. He assumes it as proved that variations in structure are all acquired; that is, are due to "use and stimuli;" and it is then easy to show that they have been transmitted from one generation to another. He also says, "all characters now congenital have been at some period or other acquired;"[1] which may or may not be true, according to the meaning we attach to the word "acquired." He gives several examples of use-inheritance among domesticated animals; such as the American trotting-horses, and the breeding of small races of dogs; but they are not convincing, owing to the complications brought about by artificial selection; while, in the cases of wool differing in different localities, and the hoofs of horses differing on hard and soft land, there is no evidence that these can be transmitted to offspring living under different conditions.

It is the same with the numerous instances brought forward of acquired characters in plants, and with the experiments showing that the embryos of the land

[1] l. c., p. 401.

Salamander (*S. atra*), when placed in water, developed new gills suitable for aquatic respiration. When the characters are undoubtedly acquired, there is no evidence that they can be transmitted to offspring living under different conditions; and, when they can undoubtedly be transmitted, it is always doubtful whether the characters are acquired—that is, rapidly brought about by external conditions—for they are all useful, and may be due to selection.[1]

It is generally allowed that, with man, children sometimes have the habits of their parents. This may occasionally be due to imitation, but not, I think, always. The jerking movement of the tails of many birds, and the side movement in that of Wagtails, are also probably inherited habits; for they do not appear to be of any use. These are not specific characters; but, if habits and instincts, which have certainly been acquired, can be transmitted, it is probable that physical characters can be transmitted also. The best instance of this is, I think, the eyes of flat-fishes, already mentioned; and, until some better explanation can be found, we must assume that this is a case of use-inheritance. In *Rhombus* and *Solea* the movement has become completely congenital, and always takes place in the same way; but in *Pleuronectes* the movement is not yet completely fixed, and takes place, sometimes on the right side, sometimes on the left, probably by the effort of the individual.

The changing colours of chrysalids also seems to be a case of the inherited action of external conditions; but it has come about very slowly, under the influence

[1] See "Are the Effects of Use and Disuse Inherited," by W. P. Ball (1890).

of natural selection, and is only an example of the indirect action of the environment.

Returning now to Mr Henslow's hypothesis of the origin of bilateral flowers, we know that galls on leaves are growths due directly to the irritation caused by insects. They are never inherited, probably because it is only a minority of the leaves of a tree which bear galls, and because the galls are irregularly placed, occurring on different leaves in different years; so that any influence they may exert on the germ-cells is counteracted by the leaves and parts of leaves which bear no galls. But with flowers the case is different. Every flower on a plant is visited several times a day by insects; and all the insects act in nearly the same manner, and irritate nearly the same parts. On the other hand, there are not the numerous physiological processes going on in the perianth as there are in the leaves; consequently, they would not be so sensitive to irritation. Also flowers are formed in the bud, and subsequently expand rapidly by interstitial growth; they last a very short time, only a few days; consequently, the reaction of the protoplasm, if it takes place at all, must take place quickly, and before the perianth has finished growing. If, therefore, the fact that galls are not inherited does not imply that hypertrophy in flowers cannot be inherited, so also the fact that insects produce hypertrophy in leaves does not prove that they do so in flowers. The subject seems to be too complicated to admit of decisive evidence one way or the other.

I think we have a better case of use-inheritance in the following. The small, coriaceous, imbricate leaves, found in many Alpine plants in New Zealand,

appear to be a truly acquired character, brought about quickly; for in *Olearia nummularifolia*, variety *cymbifolia*, the leaves on new shoots revert to the ordinary form if the plant is removed to the low land; thus showing that the peculiar shaped leaf is an acquired character and is not inherited. The Alpine *Veronica lycopodioides* has much the same shaped leaf as the Alpine variety of *Olearia*; and we have, therefore, every reason to suppose that it also is an acquired character. But, when the Alpine *Veronica* is grown at a low level, the leaves do not change; and we must, therefore, assume that an acquired character has here become congenital.

Mr Henslow gives another good example. He says, "These differences are well seen in the tendrils of *Ampelopsis hederacea*, as compared with those of *A. veitchii*. In the former there are no traces of the adhesive 'pads' at the terminations of the slender hooked tips of the branching tendrils, until contact with the surface of a wall has occurred. On the latter species, however, the pads are in course of development before any contact has taken place; just as the aërial roots of Ivy begin to appear before contact. It is, therefore, reasonable to conclude that the effect of contact has become more or less hereditary in the latter Japanese species, though not in the American."[1]

CONCLUSIONS AND SUGGESTIONS

We have seen that use-inheritance, or Kinetogenesis, cannot originate, it can only develop, already existing organs; so that, as an explanation of the origin of species, it goes no deeper than natural selection. But

[1] "The origin of floral structures," p. 156.

use-inheritance can, at best, only explain certain structures, while others remain unexplained; and there is evidence that in many cases it is ineffective. Also, nearly all the structures can be explained by indefinite variation and natural selection; so that, in most cases, use-inheritance is unnecessary. The only cases I can find in which indefinite variation seems to fail are those of the eye in flat-fishes, and the tendrils of *Ampelopsis*; and, until some other explanation of these is forthcoming, we must consider use-inheritance as a true cause; but one the importance of which has been greatly exaggerated by Lamarckians. We can only admit its influence where the evidence appears to be conclusive.

Panmixia cannot cause degeneracy; and the principle of compensation of growth is an unproved hypothesis of a very doubtful character; consequently, disuse-inheritance seems to be necessary to explain many vestigial organs; but there is no evidence to show that changes in form can be thus produced. Degenerate organs get smaller, but do not change their form; and we have positive evidence that great activity does not necessarily produce change in shape.

Physiogenesis is certainly a true cause of variation; but these variations are not transmitted to other generations, unless the same variation has been impressed over and over again on many successive generations. When this is the case, some kind of impulse seems to be transmitted to the germ-cells; but the action is slow. Characters may be suddenly acquired, but they cannot be suddenly transmitted. The origin of variations, however, is not explained by saying that they are due to the action of the

environment. We can never understand the origin of variations until we can explain the laws of growth and inheritance; neither of which are touched by Lamarckism any more than by Darwinism.

It seems probable that the energy of inheritance must have been slight in the first organisms, and have increased with repetition; for this is the teaching of a large number of experiments and observations on new varieties. But we learn from palæontology that variation was slow at first, and that it became more rapid as the earth grew older; so that the causes which induced it must have increased in intensity. This may be chiefly due to the increasing complexity of the germ-cells; but to account for this increase we must call in the continued action of the environment on the individual to supply the necessary energy.

The most reasonable hypothesis appears to be that the physico-chemical forces affect, in time, the germ-cells; and that the changes thus produced become congenital variations, capable of being transmitted to future generations. These slowly acquired variations compete in the germ-cells with variations acquired by the ancestors of living individuals, which are the results of external conditions on previous generations, and known as constitutional causes, or specific constitution; and this is at the present time of far greater importance in the production of variations than external conditions. Thus numerous complications arise in the germ-cells; and the usual result is what we call indefinite variation. These indefinite variations may become definite through repetition; and are controlled in their development by the principle of selection, sometimes aided by use-inheritance.

These were the views held by Darwin, who never thought that natural selection was the sole explanation of adaptations, and always recognised the influence of external conditions in the production of varieties. But they are very different from the teachings of the Neo-Lamarckians, who hold that use-inheritance is the chief factor in evolution. They also differ from the views of Dr Wallace and Professor Weismann, who will not allow that somatogenesis is a true cause at all; as well as from those of Professor Nägeli, who thinks that varieties arise from internal strains during growth in definite directions "towards greater perfection, that is towards greater complexity," by what he calls the "principle of improvement."

There still remains the difficulty of explaining how great changes took place in the first pelagic organisms, notwithstanding the uniformity of the conditions under which they existed. These organisms may have been numerous from the first, but they cannot have been very dissimilar. No doubt they were all endowed with the powers of assimilation, contractility and irritability, but not with the power of secreting different solid substances—such as silica, carbonate of lime, cellulose, &c.—out of their food. This power came later, and was something quite new. Different chemical actions began in different organisms, all of which were living in the ocean. The floating plants, as well as the Radiolarians and Foraminifera, lived under apparently identical conditions, while the sponges were subject to greater pressure on the bottom of the sea.

It is difficulties such as these which have led some biologists to think that the original protoplasm not

only had the property of being altered by the action of external stimuli, but that it had in addition "inherent in it, the capability of development, that is the capability of variation, and the ability to retain the results of this variability as new qualifications."[1] But the facts of palæontology are inconsistent with this view; for they show us that very many organisms have gone on perpetuating their forms for period after period in geological history, without showing any change. This could not have been the case if protoplasm contained an internal force urging it to vary; and the hypothesis just mentioned must mean that, or else it is merely a statement of the fact that organisms are capable of varying.

As a possible way out of the difficulty I would suggest that the first variations were due to different organisms assimilating different substances with their food. It is possible, even probable, that the primæval ocean was not so uniform in composition as it is now. Many chemical reactions would be going on in the crust of the earth, and different gases might be discharged in great quantities in different places; and it is possible that imbibition of these—some by one set of organisms, some by another set—may have given rise to the different forms of secretion which followed.

However this may be, we know of nothing capable of initiating organic changes, except the action of external forces on protoplasm; while the *modus operandi* remains a mystery.

[1] "Text-book of Botany," by Strasburger, Schimper, Noll and Schenck, English Translation by Porter, p. 4 (1898).

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